Journal

of

THE ROYAL AERONAUTICAL SOCIETY

WITH WHICH IS INCORPORATED THE INSTITUTION OF AERONAUTICAL ENGINEERS

A Monthly Illustrated Journal devoted to all subjects connected with the Profession of Aeronautics

EDITED FOR THE COUNCIL OF THE
ROYAL AERONAUTICAL SOCIETY

by

JOAN BRADBROOKE, Associate

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of

THE ROYAL AERONAUTICAL SOCIETY

JULY 1956

CONTENTS

NOTICES

THE FORTY-FOURTH WILBUR WRIGHT MEMORIAL LECTURE—THE AERODYNAMIC ART — SIR WILLIAM S. FARREN

INTERNATIONAL AIRWORTHINESS STANDARDS — SIR FREDERICK TYMMS

AEROELASTIC PROBLEMS IN CONNECTION WITH HIGH-SPEED FLIGHT — E. G. BROADBENT

RECENT IDEAS IN PLASTICS FOR AIRCRAFT STRUCTURES — J. E. GORDON

NEW ZEALAND DIVISION—SEVENTH ANNUAL REPORT

TECHNICAL NOTES

The Compression of Circular Rings — W. JOHNSON. Buckling of Rectangular Plates with One Unsupported Edge, Compressed by Forces Distributed Along its Edges—P. SHULESHKO. Reciprocation of Triply Partitioned Matrices—E. KOSKO. Determination of Velocity and Pressure Distribution Along the Surface of Annular Aerofoils with Thick Symmetrical Section—S. PIVKO.

GRADUATES' AND STUDENTS' SECTION

THE LIBRARY — REVIEWS — REPORTS

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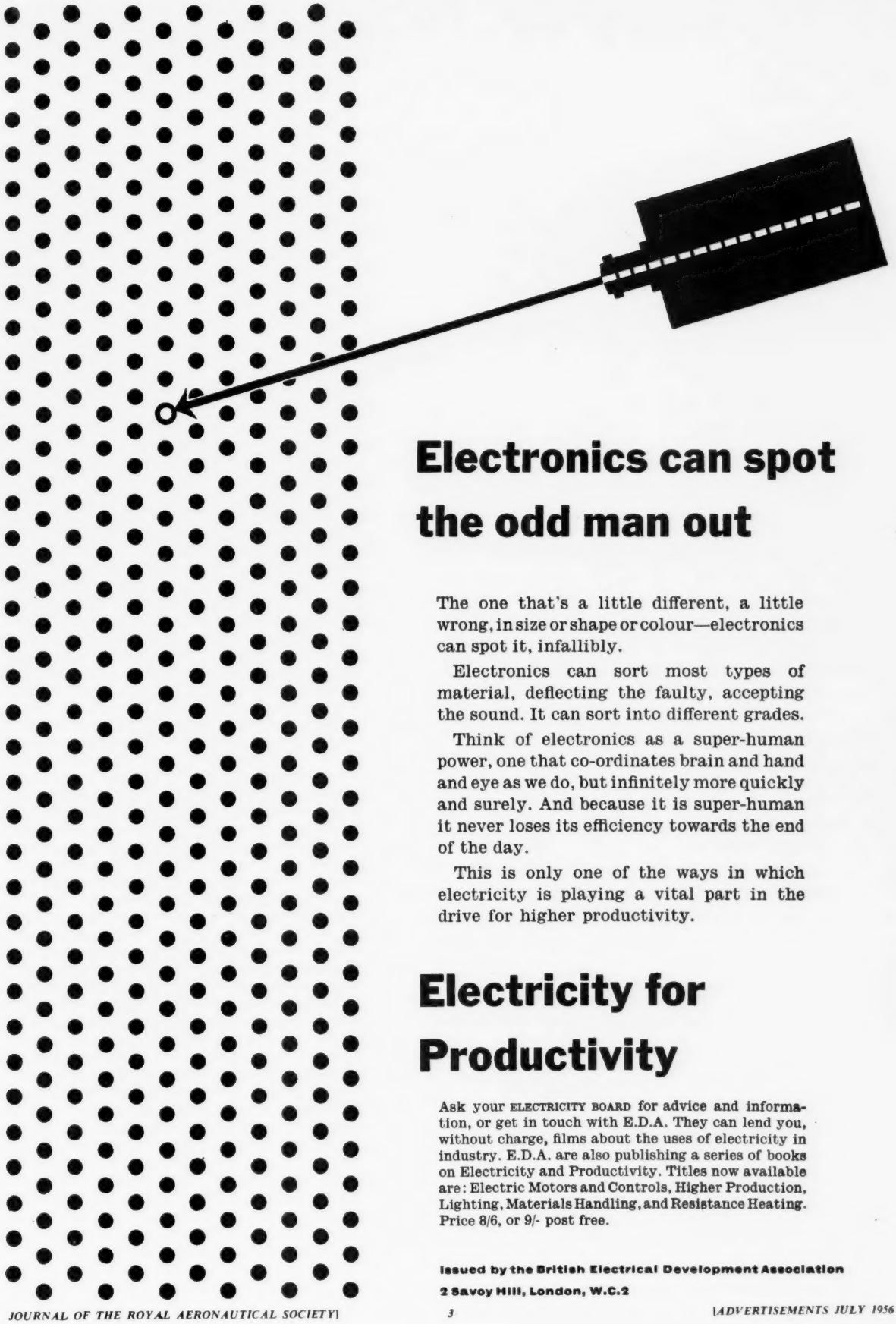

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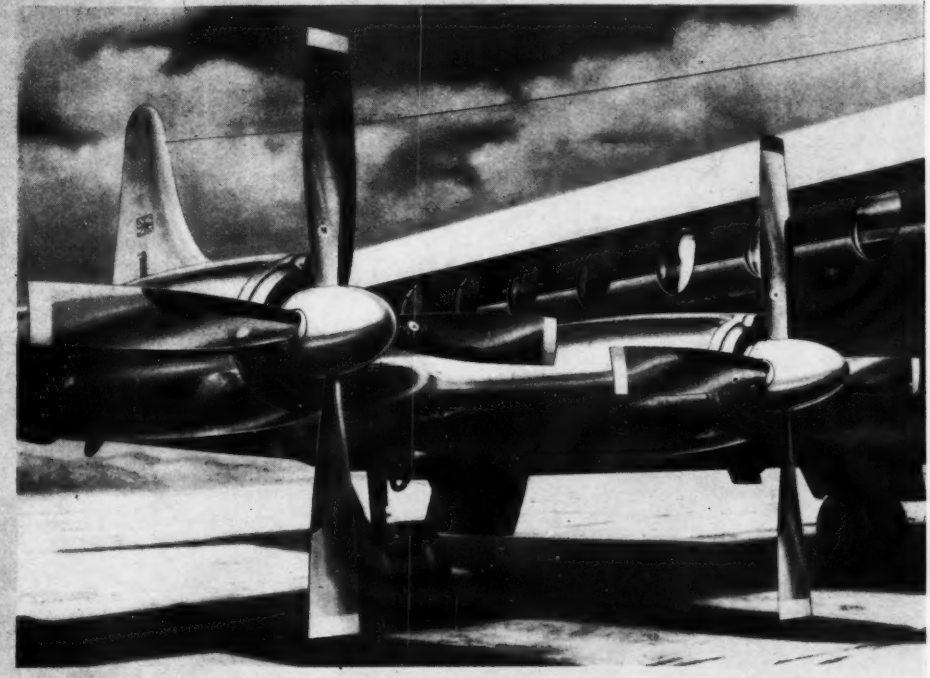

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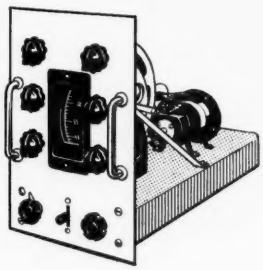

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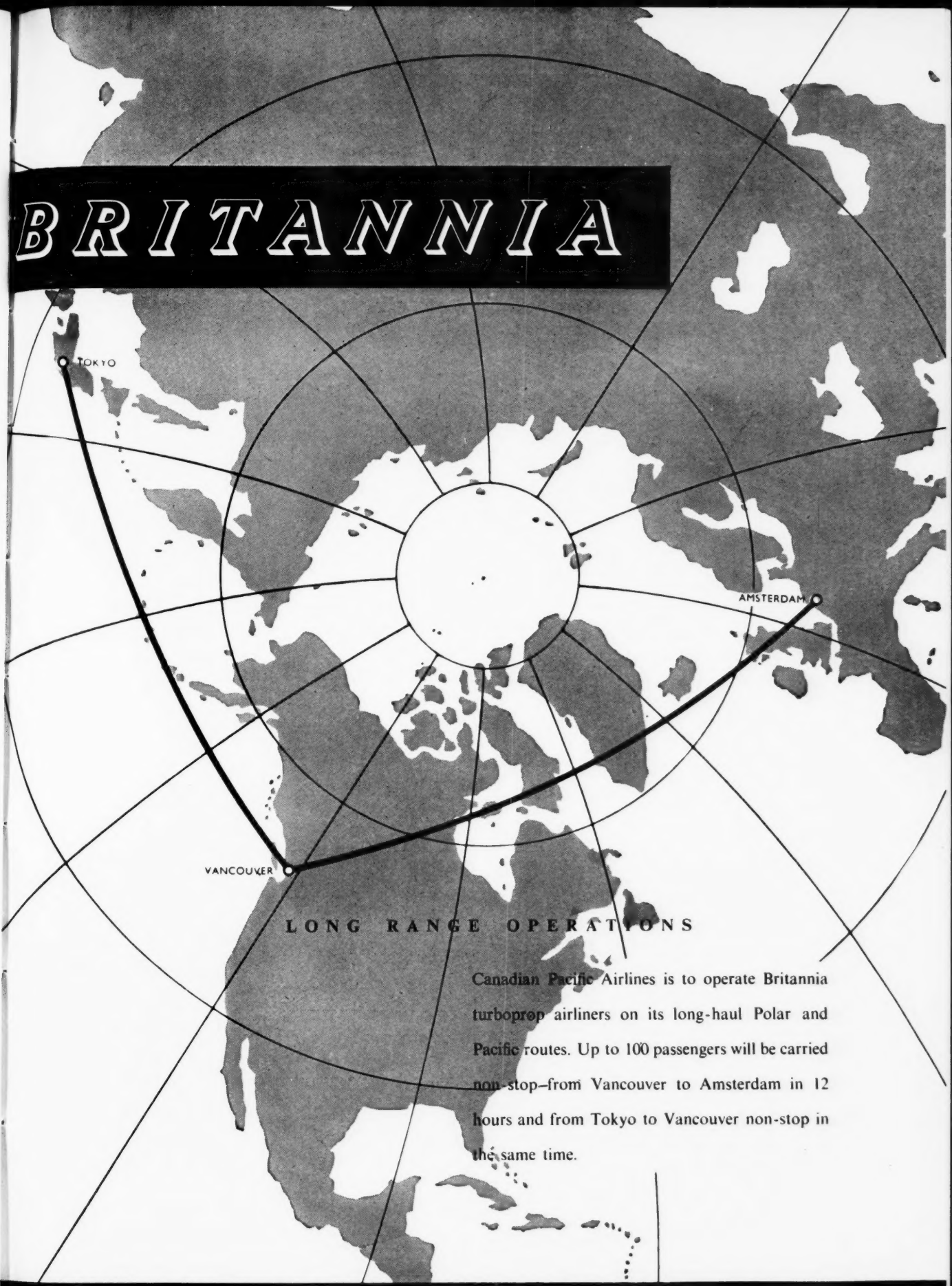

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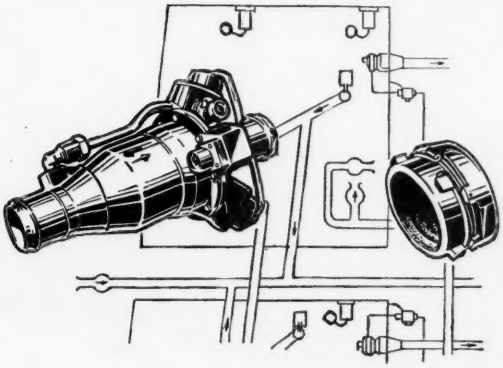

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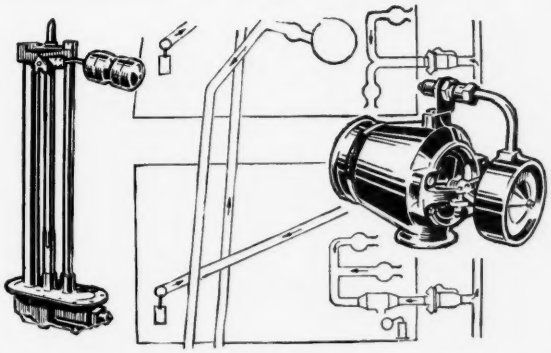

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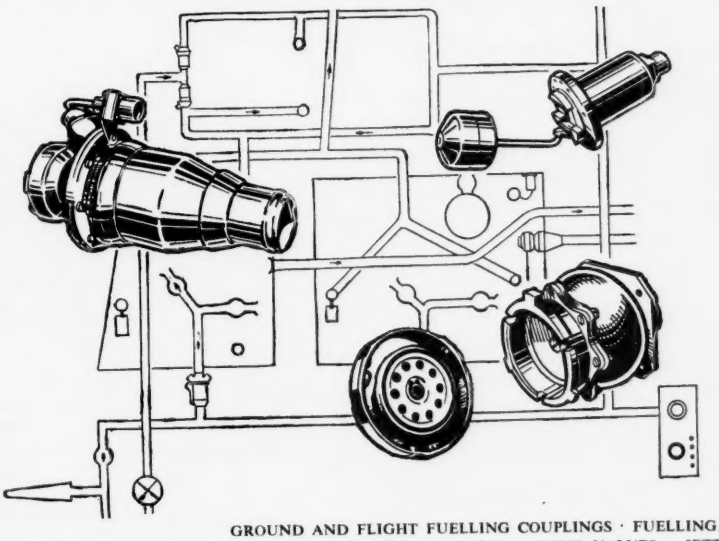

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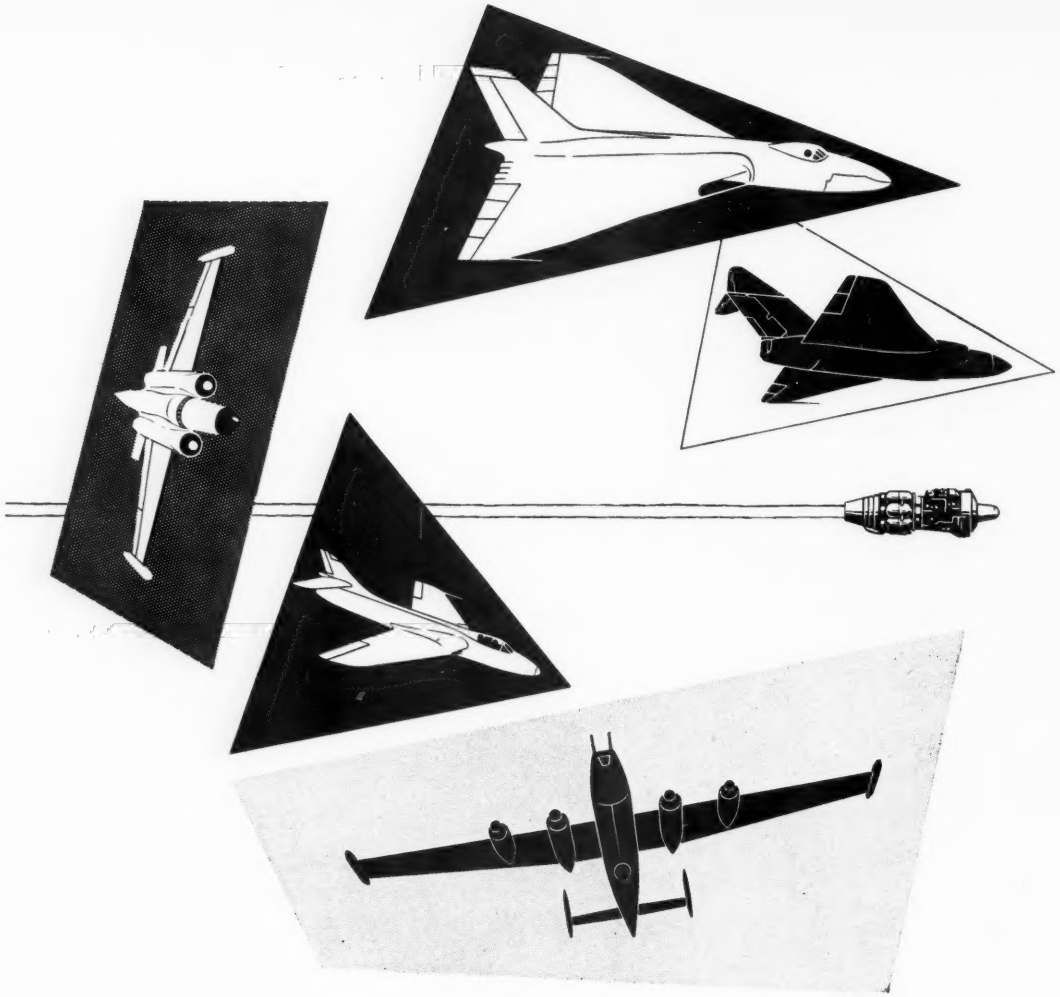

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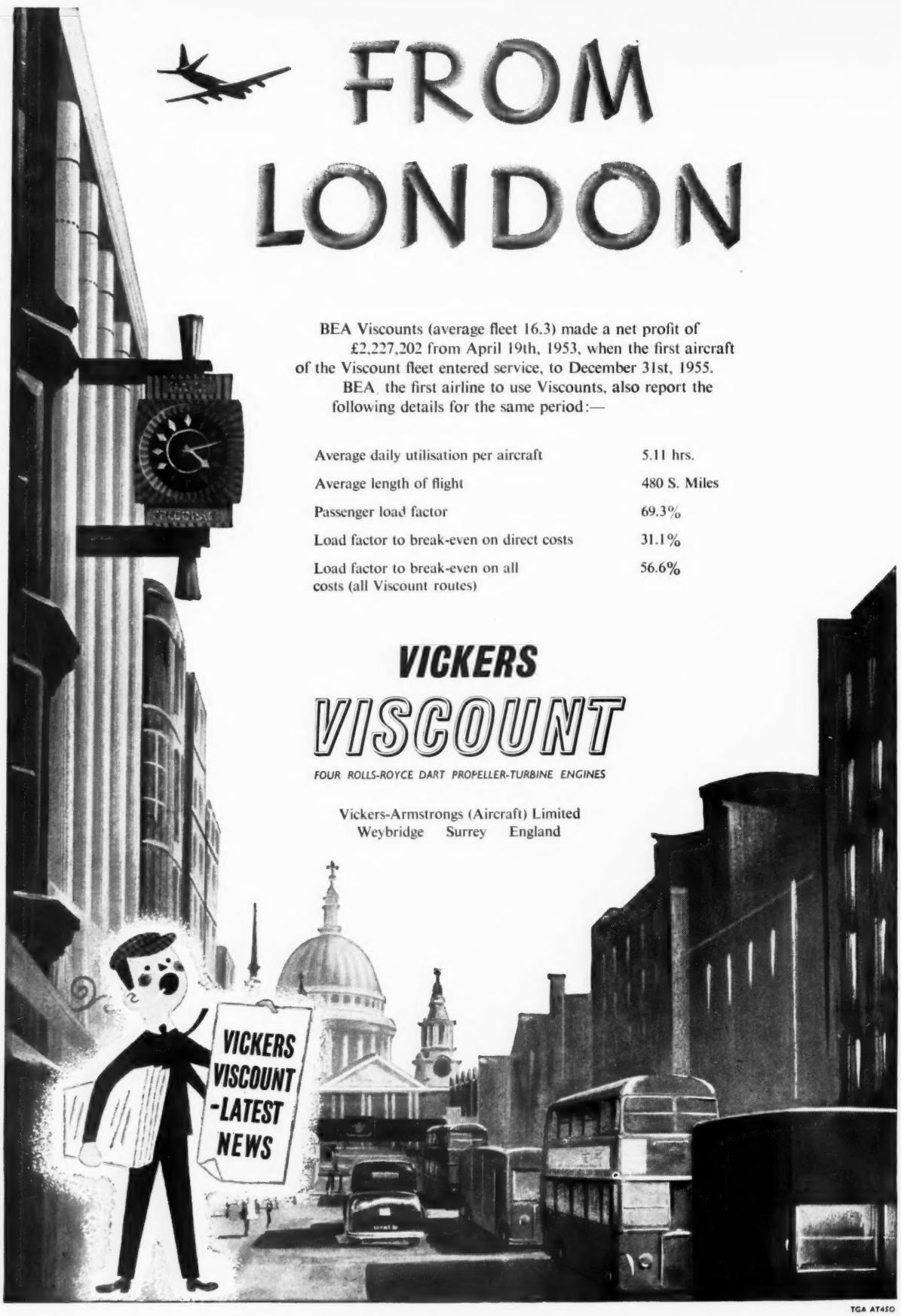

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VOLUME 60 JULY 1956 NUMBER 547

CONTENTS

PAGE

Notices XLI

School Holidays in France Exchange

August Bank Holiday—Associate Fellowship Examination—Garden Party, 15th July 1956—Birthday Honours 1956—Wakefield Memorial Scholarships—Elliott Memorial Prize 1956—Joint Conference with the Stress Analysis Group of the Institute of Physics—News of Members—Elections—Changes of Address or Appointment—Anglo-American Conference Proceedings—Presentation of Awards.

The Forty-Fourth Wilbur Wright Memorial Lecture—The Aerodynamic Art — SIR WILLIAM S. FARREN — 429

International Airworthiness Standards — SIR FREDERICK TYMMS — 450

Aeroelastic Problems in connection with High-Speed Flight — E. G. BROADBENT — 459

Recent Ideas in Plastics for Aircraft Structures — J. E. GORDON — 476

New Zealand Division—Seventh Annual Report 482

Technical Notes 484

The Compression of Circular Rings—W. JOHNSON. Buckling of Rectangular Plates with One Unsupported Edge, Compressed by Forces Distributed Along its Edges—P. SCHULESHKO. Reciprocation of Triply Partitioned Matrices—E. KOSKO. Determination of Velocity and Pressure Distribution Along the Surface of Annular Aerofoils with Thick Symmetrical Section—S. PIVKO.

Graduates' and Students' Section 492

The Library 493

REVIEWS: Surveys in Mechanics—Introduction to Plasticity—Theory of Combustion Instability in Liquid Propellant Rocket Motors—Tragflügeltheorie: Inkompressible Flüssigkeiten—Plane Waves and Spherical Means Applied to Partial Differential Equations—Methods of Mathematical Physics—Weltraumstrahlung (Cosmic Radiation)—Practical Metrology, Volumes 1 and 2—The Observer's Book of Aircraft—A Picture History of Flight.

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None of the papers or paragraphs must be taken as expressing the opinion of the Council of the Royal Aeronautical Society unless otherwise stated.

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NOTICES

An annual sum of £250 is available for premium awards for papers, including Technical Notes, published in the Journal. Members and non-members of the Society are invited to submit papers on any aspect of aeronautics

August Bank Holiday

The Library and Offices of the Society will be closed from 5 p.m. Friday 3rd August until 9 a.m. on Tuesday 7th August.

Associate Fellowship Examination

The closing date for Candidates in the United Kingdom for the December 1955 Examination in Part II in the Old Syllabus is 31st August. The entry list for those residing abroad closed on 31st May. For those taking the Revised Syllabus the closing date was 30th June. Entry forms for the old Syllabus may be obtained from the Secretary.

Wakefield Memorial Scholarships

Two annual scholarships in memory of Viscount Wakefield, late President of the College of Aeronautical and Automobile Engineering, have been founded and are maintained by Messrs. C. C. Wakefield Ltd. They are for the training of one student to the standard of the A.M.I.Mech.E. examination in Automobile Engineering and one student to the standard required by the City and Guilds of London Institute Department of Technology in Agricultural Engineering. Application forms may be obtained from the Principal, The College of Aeronautical and Automobile Engineering, Sydney Street, London, S.W.3.

Elliott Memorial Prize 1956

The Elliott Memorial Prize has been awarded to 589047 Corporal Apprentice D. Holcroft who was the aircraft apprentice of the September 1953 Entry who obtained the highest marks in the General Studies Examination. His prize will be presented to him at the Graduation Prize Giving which is due to take place on 1st August 1956.

News of Members

H. G. Anthony (*Associate Fellow*), formerly with Air Service Training, Hamble, has taken up a post with Marshalls of Cambridge as a Project Designer.

Squadron Leader W. A. Beedie (*Associate Fellow*), formerly Technical Engineer, R.A.F., S.T.S.O. Branch, H.Q.B.F., Aden, has recently joined the Staff of the Air Ministry (Air Eng. 4b).

Air Chief Marshal Sir Ralph Cochrane (*Associate Fellow*) is to take charge of the administration of Nuclear Research for Rolls-Royce Ltd. at Derby.

J. S. Collins (*Associate Fellow*) has left Lucas Rotax Ltd., Ontario, and has joined the Design and Development Group, Aeronautical Division of Minneapolis Honeywell Regulator Co. at Toronto.

R. M. Cracknell (*Associate Fellow*), formerly Engine Development Manager of B.E.A., has now joined de Havilland Propellers Ltd. as Assistant Engineering Manager.

Wing Commander A. E. Davies (*Associate Fellow*) has left the Directorate of Air Navigation to take up an appointment at the Directorate of Engine Research and Development, Ministry of Supply.

J. D. Davis (*Associate Fellow*) is now Manager, Products Divisions, Oil Products Development Department, Shell Petroleum Co. Ltd.

W. C. Dee (*Associate*) has taken up a new appointment with E.M.O. Instrumentation Ltd. as Technical Sales Engineer.

Garden Party 15th July 1956

Members are reminded that the Society's Garden Party is being held on 15th July 1956 at the Vickers-Armstrongs Aerodrome at Wisley, Surrey from 2.30 p.m. until 6.00 p.m. Tickets (adults 10s. 6d., children 5-15 years 5s. 6d.) are for members and their guests only. Tickets will not be available at the Aerodrome.

The President, Mr. E. T. Jones will receive members and their guests at 2.30 p.m. and there will be a flying programme from 3.00 until 5.30 p.m.

The car park will be open from 12 noon but LUNCH WILL NOT BE AVAILABLE AT THE AERODROME.

Birthday Honours, 1956

Among Members of the Society who were honoured by Her Majesty in the Birthday Honours List were the following:—

Knight Commander of the Bath:
Air Vice-Marshal T. N. McEvoy (*Associate Fellow*)

Knight Bachelor:
Mr. H. W. Clarke (*Fellow*)
Mr. G. H. Dowty (*Fellow*)

Imperial Service Order:
Mr. C. Howarth (*Fellow*)

Order of the British Empire
Mr. L. Boddington (*Fellow*)

School Holidays in France

The Secretary has a list of Members of A.F.I.T.A. in France who have children of school age who would like to exchange holidays with children, between the ages of 12 and 17 (boys and girls) of members of the Society. Full particulars will be sent on application to the Secretary, but as some of the suggested holiday dates are **very** near early application should be made.

R. Fisher (*Associate*) has left Canadair Ltd. in Montreal and is now a Design Engineer with Republic Corporation in New York.

G. F. Gilmore (*Associate Fellow*) has taken up an appointment as Senior Lecturer in Mechanical Engineering at Hendon Technical College.

B. S. Grover (*Graduate*), formerly in the London Design Office of the Bristol Aeroplane Co., is now a Stressman with Auster Aircraft Ltd.

Noel Holman (*Associate*) has been appointed the Southern Africa Representative of The Bristol Aeroplane Co. Ltd., in succession to Air Commodore J. C. Wake.

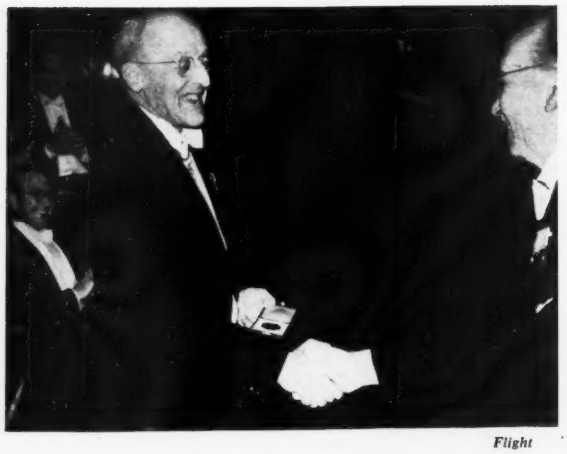
Viscount Knollys (*Companion*) has succeeded Sir Ronald Weeks as Chairman of Vickers Ltd.

D. M. Lambert (*Associate*) has left Armstrong Siddeley Motors Ltd., Coventry, and is now a Director of Clark & Lambert Ltd.

J. P. McCormick (*Associate*) has been appointed Personnel Manager to Douglas (Kingswood) Ltd. which is a Division of the Westinghouse Brake & Signal Co.

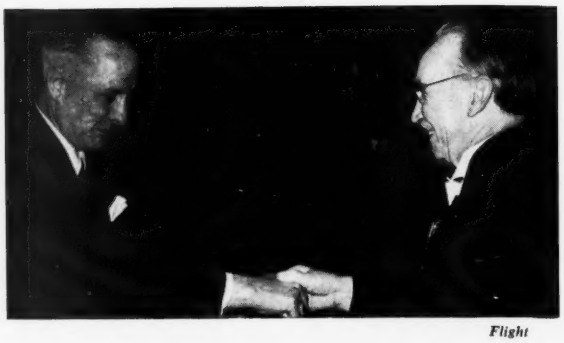
W. D. S. Marshall (*Associate Fellow*) has left the Aircraft Research Association and has joined Test Equipment (Models) Ltd. as Assistant Chief Engineer.

Presentation



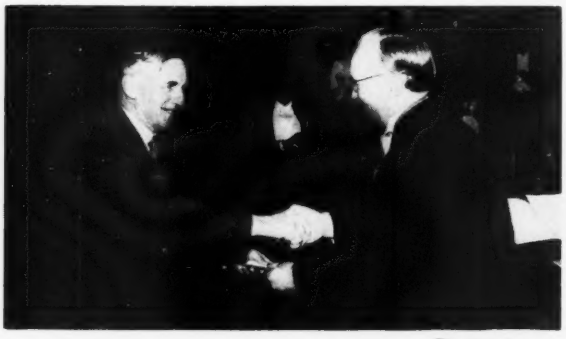
Flight

The Medals of the Society awarded by the Council were presented at the 44th Wilbur Wright Memorial Lecture on 17th May 1956, by the President, Mr. E. T. Jones, C.B., O.B.E., F.R.Ae.S. In this photograph he is presenting the Society's Gold Medal to Sir William S. Farren (*Fellow*), Technical Director, A. V. Roe and Co., Ltd., and Past-President of the Society, for his "outstanding contributions to Aeronautical Research and Development." Sir William gave the 44th Wilbur Wright Memorial Lecture.



Flight

Dr. E. S. Moult (*Fellow*), Director, The de Havilland Engine Co., Ltd., and a Member of Council, receiving the Society's Silver Medal for his "outstanding contributions to Aeronautical Engineering and, in particular, for his work in the Design of Aircraft Engines."



The Aeroplane

Mr. J. W. Barnes, Chief Research Engineer, Aircraft Equipment Dept., Ferranti Ltd., receiving the Society's Bronze Medal for his "contributions in the field of Aircraft Instruments."

Flight

Group Captain W. K. Stewart, Officer Commanding the R.A.F. Institute of Aviation Medicine, receiving the Wakefield Gold Medal for his "contributions to the Design and Development of Aero-Medical Equipment."

Flight

Mr. David Smith (*left*) receiving the British Gold Medal for Aeronautics which was awarded posthumously to his father, Mr. Joseph Smith, who was Chief Designer, Supermarine Division of Vickers-Armstrongs, from 1937 until his death in February 1956, for his "outstanding practical achievements in the Design and Development of Military Aircraft." *Centre* Mr. K. V. Diprose, A. V. Roe and Co. Ltd., receiving the Simms Gold Medal for his paper on "Analogue Computing in Aeronautics." *Right* Mr. R. H. Woodall (*Fellow*), Technical Director, Rotax Ltd., receiving the George Taylor (of Australia) Gold Medal for his paper "Some Trends in the Development of Aircraft Electrical and Starting Systems."

# of Awards

Flight

Mr. O. N. Lawrence (*Associate Fellow*), Technical Director, Joseph Lucas (Gas Turbine Equipment) Ltd., receiving the Herbert Akroyd Stuart Memorial Prize for his paper "Fuel Systems for Gas Turbine Engines."

Flight

Mr. D. P. Davies, Chief Test Pilot, Air Registration Board, receiving the R. P. Alston Memorial Prize for his "practical achievement in the flight testing of prototype civil aircraft."

Flight

Dr. G. N. Lance (*Graduate*), Lecturer in Applied Mathematics, University of Southampton, receiving the Usborne Memorial Prize for his paper on "The Lift of Twisted and Cambered Wings in Supersonic Flow."

Flight

Prizes of the Society awarded by the Council were presented at the Annual General Meeting of the Society held on 10th May 1956 at 4 Hamilton Place, by the retiring President, Mr. N. E. Rowe, C.B.E., F.R.Ae.S. Mr. Walter Tye, Chief Technical Officer, Air Registration Board, is here receiving the Branch Prize for his paper "The Outlook on Airframe Fatigue" (the Second Barnwell Memorial Lecture).

Flight

Mr. K. Gunn, Aluminium Laboratories Ltd., receiving the Orville Wright Prize for his paper on "Effect of Yielding on the Fatigue Properties of Test Pieces containing Stress Concentrations."

Flight

Mr. John Grierson, de Havilland Engine Co. Ltd., receiving the Royal Aeronautical Society Navigation Prize for his paper "The Evolution of Arctic Airways."

News of Members—*Continued*

R. J. Millson (*Associate Fellow*) has been appointed Head of the Publication Department of the Institution of Mechanical Engineers.

G. T. O'Brien (*Graduate*) has left Short Bros. & Harland Ltd. and is now an Aerodynamicist "A" in the Project Department of Avro Aircraft Co. Ltd. in Ontario.

H. E. Parish (*Associate Fellow*) has been appointed Assistant Chief Stressman of Hunting Percival Aircraft Ltd.

A. Powell (*Associate Fellow*) has resigned his position as Lecturer in the Aeronautical Engineering Department of Southampton University to take up an appointment as Research Fellow in the Guggenheim Laboratory, California Institute of Technology, where he will also be a consultant to Douglas Aircraft Co. Inc.

J. S. Rivaz (*Associate*), formerly Technical Services Manager to Kelvin & Hughes (Aviation) Ltd., has been appointed Technical Sales Manager at Smith Aircraft Instruments Ltd.

B. S. Shenstone (*Fellow*) has been made a Fellow of the Canadian Aeronautical Institute.

T. W. Shreeve (*Associate*) has been appointed Design Section Leader at the London Office of Dowty Equipment Ltd.

W. Smith (*Associate Fellow*) has resigned his appointment with A.W.R.E. Atomic Energy Authority at Aldermaston and has been appointed Chief Project Engineer with the Hawker Siddeley Nuclear Power Co.

Professor R. H. Upson (*Fellow*), formerly at the University of Minnesota Institute of Technology, is now with Boeing Airplane Co. in Seattle.

R. I. Vaughan (*Graduate*) has recently resigned his position as Senior Scientific Officer at the R.A.E. to join Rolls-Royce Ltd.

Air Commodore J. H. C. Wake (*Associate Fellow*), Southern African Representative of the Bristol Aeroplane Co. Ltd. since December 1949, is returning to this country to take up an appointment with the Helicopter Department of Bristol Aircraft Ltd.

R. F. West (*Associate Fellow*) has terminated his appointment as S.S.O. with the Ministry of Supply and is now a Defence Service Scientific Officer with the Defence Research Board of the Canadian Government at Valcartier, Quebec.

Stanley E. Willis (*Associate*) has left Canadair Ltd. in Montreal and is now a Mechanical Designer with Lear Incorporated at Santa Monica, California.

P. J. Windibank (*Associate Fellow*) has left the Royal Air Force and is now in the Publicity Department of Saunders-Roe Ltd., Cowes.

Anglo-American Conference Proceedings

The Proceedings of the Fifth Anglo-American Aeronautical Conference, which was held in Los Angeles in June 1955, are now available from the offices of the Society, 4 Hamilton Place, London, W.1, at £6 6s. 0d. per copy, including postage and packing.

The volume contains the 18 complete papers and discussions presented during the technical sessions of the Conference.

Changes of Address or Appointment

To assist in keeping the records of members correct and up to date the Secretary will be glad if all members will notify him as soon as possible of changes of address. He would also like to know of any change of appointment.

When notifying changes please give the following particulars:—

Name (in block letters). Grade of Membership.
New address (in block letters). Old address.

New appointment—Please give name and address of employer and position held.

Changes of address must be received *before the 15th of the month* in order to be effective for the Journal for the following month.

Elections

The following is a list of new members and transfers of membership of the Society:—

Associate Fellows

Roy Benstead (*from Graduate*)
David Richard Blundell (*from Graduate*)
Lewis Frederick Crabtree (*from Graduate*)
Arthur John Davis
Harold Maurice Dean (*from Graduate*)
Richard Dean
Edward Mark Dowlen (*from Graduate*)
Douglas William Eggleton (*from Graduate*)
Derek Peter Douglas Ferguson
Frederick James Fuell (*from Graduate*)
Robert John Gladwell (*from Graduate*)
George Ian Livingston Gunn (*from Graduate*)
Bernard Stephen Hansom (*from Graduate*)
Cecil John Hart (*from Graduate*)
Edward Albert Cecil Holmes
Henry Trevor Howard
Graham Basil Hustings (*from Graduate*)
Bernard Patrick Kane
Frederick Claude Kinchela (*from Graduate*)
W. Trevor Lord
Alfred John Murphy
William Turnbull Neill (*from Associate*)
Kenneth William Newby (*from Graduate*)
Keith Jones Oldham (*from Graduate*)
Wilfred George Paskins (*from Graduate*)
Bernard John Preston (*from Graduate*)
John Vivian Stanbury (*from Associate*)
John Michael Lawson Thomas (*from Graduate*)
Gordon Gresley Wakefield
James Lanham Wildhaber (*from Graduate*)
Clifford A. Williams (*from Associate*)
Louis Basil Zambon

Associates

Albert Jack Baker
Robert Fawcett Little
Norman Ernest Cyril Maggs
Mohammad Anwar Rehani
Maurice George Russell
John Ernest Smith
Cyril Frederick Whittaker
Stanley Guy Wilkinson

Graduates

Thomas Glenville Doe
David Michael Lawrence
Dennis Brigham Leason
Kenneth McQuade
Dennis John Miles
Robert William Moore
George Herbert Mycroft
Harry William Pakes
Leonard Pennelegion
David Clive Drummond Potter
Brian John Powell
Pamela Mary Smith (*from Student*)
Peter James Treweek
John Walton
Lionel Joseph Williams
Peter Vaughan Williams

Students

Derek William Altham
Colin Barton
Antony John Beaumont
William Frederick Bennett
Andrew Bialkowski
Nigel Ben Carruthers
Woodford Kenneth George Causer
Derek Charles Cook
James Ian Croft
Amar Singh Dhillon
Brian Malcolm Dobbie
Pamela Judith Draper
Ivor Brian Foster
Peter Charles Gooderham
John Grimson
Gregory Redvers Hargrove
Joseph Irwin Harris
Maxwell John Holland
Anthony Challenor Jackson
Peter Nigel Jarvis
Maxwell Brian Kelly
Robert Francis Kirkby
John Lambert
Royston John Long
Robert John McDowell
John McQueen
Christopher John Marsh
John Vincent Maude
John Brian Milburn
Peter Myers
Peter Frederick Neal
Jonathan Patrick Orme
John Edward Pitkin
Michael John Plummer
Ralph Stuart Pointer
Clifford Potter
Mandakulathur Krishnamurthy Ramamurthy
David Malcolm Richards
Anthony de Montjoie Rudolf
Raymond Cecil Scott
Victor Edward Walter
Charles Alan Wells
Peter Maurice Glynn Williams
Geoffrey Edgar Frank Young

The Journal of the Royal Aeronautical Society

WITH WHICH IS INCORPORATED THE INSTITUTION OF AERONAUTICAL ENGINEERS

VOLUME 60 JULY 1956 NUMBER 547

THE FORTY-FOURTH WILBUR WRIGHT MEMORIAL LECTURE

THE FORTY-FOURTH Wilbur Wright Memorial Lecture, "The Aerodynamic Art," was given by Sir William S. Farren, C.B., M.B.E., M.A., D.Sc., F.R.S., M.I.Mech.E., Hon.F.I.A.S., F.R.Ae.S., on 17th May 1956 at the Royal Institution, Albemarle Street, W.1, before a large and distinguished audience.

Mr. E. T. Jones, C.B., O.B.E., M.Eng., F.R.Ae.S., President of the Society, presided at the meeting. As has now become the custom, the chief medals awarded for the year by the Council were presented before the Lecture. In addition the scrolls of Honorary Fellowship were presented to the two new Honorary Fellows elected this year.

MR. E. T. JONES: This is the 44th Wilbur Wright Lecture. Now the very first lecture that was read before this Society was in the year 1866. And the first Wilbur Wright Lecture was given by Horace Darwin in 1913. Both of these dates are important in the history of our Society. There are however other important occasions too in the Aeronautical Calendar. But the year 1903 transcends them all. The men responsible for this date being important were, of course, the Wright Brothers and we meet tonight in honour of their memory through the presentation of the 44th Wilbur Wright Memorial Lecture. This particular evening has another relevant significance. There are only seven people who have given both the Wright Brothers' Lecture in America and the Wilbur Wright Memorial Lecture in this country and I note that four of those are present in this audience.

Before we proceed with the lecture I have a pleasant duty to perform. It is, as you know, our custom now to present the honours and awards that Council has made in the year on your behalf to the people who are to receive them and so I think we will proceed to that pleasant business.

The highest honour that this Society can bestow is Honorary Fellowship of the Society and I have pleasure in handing this scroll to that dynamic personality in aeronautics, Sir Roy Dobson.

Now we have another award of Honorary Fellowship and this is to a man who is probably equally known in British aeronautics although he is an American. You all know Dr. Hugh Dryden, that eminent leader and inspiring Director of the National Advisory Committee for Aeronautics who has always been a very good friend to us in the aeronautical field. Dr. Dryden is already an Honorary Member of the Institute of the Aeronautical Sciences and this will therefore perform the double occasion.

Sir William S. Farren
44th Wilbur Wright Memorial Lecturer and
1956 Gold Medallist of the Society

The highest Medal award that the Society can bestow is the *Society's Gold Medal* and although *Sir William Farren*, who is going to receive this Medal, also happens to be our Lecturer this evening, that is a pure coincidence. Sir William, I know, did not wish to receive this Medal. Being a Member of Council for years he felt that it was not playing cricket but his colleagues had him on the spot because when he himself was President three years ago and this subject was up for discussion, he concluded by ruling that it was a lot of nonsense. In future, he said, Council members should not feel sensitive on this matter, so he has not now a word to say. May I have, Sir William, the privilege of handing you this Medal.

Now the *Society's Silver Medal* this year goes to another Council member, *Dr. E. S. Moult* (*Fellow*), of the de Havilland Engine Co. Ltd., for his outstanding contributions to Aeronautical Engineering, and, in particular, for his work in the Design of Aircraft Engines.

The *Society's Bronze Medal* is also a very important award of this Society and it goes this evening to one whom we know as having worked in the aircraft instrument field for many years. I am sure you will all be glad to find that that part of aeronautical work is

Honorary Fellows, 1956—The President, Mr. E. T. Jones, C.B., O.B.E., F.R.Ae.S., presenting the Scrolls of Honorary Fellowship to (*left*) Sir Roy H. Dobson, C.B.E., J.P., F.R.Ae.S., Managing Director, A. V. Roe & Co. Ltd., and (*right*) Dr. Hugh L. Dryden, Ph.D., Hon.F.I.A.S., F.R.Ae.S., Director of the National Advisory Committee for Aeronautics.

properly recognised by this award to *Mr. J. W. Barnes*, who was for many years at the R.A.E. and is now with Ferranti Ltd.

Now the *British Gold Medal for Aeronautics* has been awarded this year posthumously to *Mr. Joseph Smith*, who was Chief Designer of the Supermarine Division of Vickers, for his outstanding practical achievements in the Design and Development of military aircraft. I have very great pleasure in handing this medal to Mr. Jo Smith's son, David.

The *British Silver Medal for Aeronautics* again goes to a field which is not quite so glamorous as some of the other fields but it is beginning to come into its own. It goes this year to *Mr. J. Fenwick*, who is in charge of the Southern Air Traffic Control Centre, for his contributions to the development of Air Traffic Control Systems.

The *Wakefield Gold Medal* is generally awarded to those who have invented or designed something outstanding in the field of safety equipment and this year the work of *Group Captain W. K. Stewart* in Aero-Medical equipment has been recognised. He is Officer Commanding the R.A.F. Institute of Aviation Medicine.

The *Simms Gold Medal* has been awarded this year to *Mr. K. V. Diprose*, formerly with the R.A.E. and now with A. V. Roe & Co. Ltd., for his paper on " Analogue Computing in Aeronautics."

The *George Taylor* (*of Australia*) *Gold Medal* is awarded to *Mr. R. H. Woodall* (*Fellow*), of Rotax Ltd., for his paper " Some Trends in the Development of Aircraft Electrical and Starting Systems."

It could not be possible to stage any aeronautical gathering to which Sir William Farren needed an introduction. It would be equally unreal for me to assume that anyone in this audience did not know that this Fellow of Trinity College and Fellow of the Royal Society, has not only been pilot, designer, aeronautical engineer, aeronautical consultant, educationalist and aeronautical lecturer, but has pursued all these aeronautical professions simultaneously over 41 years. No branch of aeronautics has escaped his attention and few honours have passed him by. Earlier tonight, as you have seen, I had the pleasure of presenting to Sir William on Council's behalf, the highest award the Society can bestow and later tonight he will join that select band who hold the double blue lecture riband of the Atlantic. I will now ask Sir William to give us the 44th Wilbur Wright Lecture.

The 44th Wilbur Wright Memorial Lecture

The Aerodynamic Art

by

SIR WILLIAM S. FARREN, C.B., M.B.E., M.A., D.Sc., F.R.S., M.I.Mech.E., Hon.F.I.A.S., F.R.Ae.S.
(*Technical Director, A. V. Roe & Co. Ltd.*)

Aerodynamics—Science and Art

Aerodynamics is the foundation on which we build aircraft, as did the Wright Brothers. In their day, it was something of a mystery. It has become a profession in itself—a Science and an Art.

Its parent science—the mechanics of fluids—is one of the most difficult branches of the general science of mechanics, which is itself possibly not one of the easier departments of science. The simplest of our flying machines is aerodynamically excessively complex. We have always been prepared to design and build aircraft to meet extraordinarily severe conditions. There is only one yardstick by which we can expect to be judged—results. We need all our skill—and skill is art, though not the whole of it.

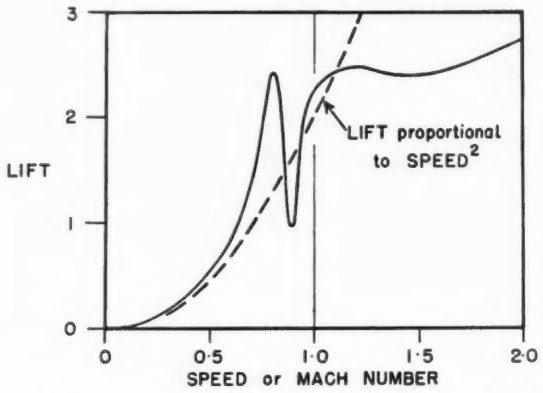
Wilbur and Orville Wright were perhaps more happily placed than we are. They were good engineers and wanted some reliable numbers to put into their estimates. They were ready to take risks—calculated risks—but since the margins available to them in weight and power were small, they had to have a fairly good idea of the forces of the air on the wings and the rest of their aircraft. They devised a wind tunnel and made measurements, which they compared with such other measurements as they could make of what their gliders did. But they judged correctly that their biggest problem would be the handling of the flying machine, and for this depended entirely on their experiments with gliders. They observed and reasoned. They studied and practised the aerodynamic art in the only laboratory available to them—the air. They followed the line of sailors from the beginning of time. We must recognise that, with all the devices we have contrived, in all our experience of half a century of flying, there still remain things to be done with a new aircraft which can be done only in flight. And what we discover by our experience in flight—the problems, as much as the answers—is the greatest single stimulus to the advance of aerodynamic science.

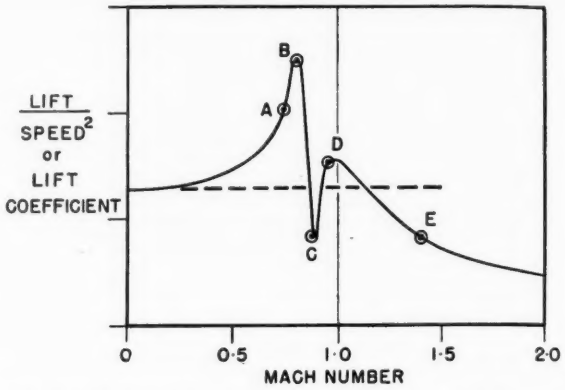
Judged by the amount of information available—the "facts," the "data"—aerodynamics has advanced prodigiously in 50 years. But then, what science has not? Has our understanding of it, our power over it, kept pace?

The first simple conception of aerodynamics distinguished two main divisions. First, the flow of air under the influence of the aircraft's passage and its relation to the forces produced on the surface of the aircraft. Second, the dynamics of the aircraft as a rigid body. The mechanics of propulsion, by propellers, was recognised by the Wright Brothers as a comparatively simple extension of the mechanics of the lifting wing. By others, even at that early date, it was recognised as a form of jet propulsion. Probably the greatest single addition which has been made to the mechanical problems of flight was due to the realisation that an aircraft is by no means a rigid body, but is embarrassingly flexible.

The almost purely empirical attitude to aerodynamics which characterised the first 20 years of flight ended with the publication of Prandtl's famous paper N.A.C.A.116 written for, and published by, the National Advisory Committee for Aeronautics of the United States of America in 1925, and of Glauert's book "Aerofoil and Airscrew Theory" published in 1926. These revolutionised our understanding and powers of analysing the main phenomena which determine the lift and drag of aircraft, the thrust and torque of propellers, and the interference of wind tunnel constraints. It is hardly too much to say that a vast mass of indigestible "facts" was rendered of historical interest by a few pages of inspired theoretical work. Prandtl's paper contained 58 pages and Glauert's book 266. In all, they amounted to about 100,000 words. The vision of Lanchester brooding over the irony of a world which at last understood what he had seen so clearly, but had been unable to explain, will not fade from the minds of those who knew him.

The significance of the boundary layer, implicit in Prandtl's treatment of lift and drag, gradually became understood. Its practical effect was most apparent in Melvill Jones's notable lectures before this Society, "The Streamline Aeroplane" (January 1929) and "Profile Drag" (December 1936), also in his Wilbur Wright Memorial Lecture, 1934, on "Stalling." Turbulence not only of the boundary layer but of the "free stream" became the preoccupation of many schools, who will probably recognise their and our own debt to G. I. Taylor and von Kármán. Laminar and turbulent boundary layers passed from the purely scientific to the engineering field, and "separation" of the flow from the streamline aircraft became recognised as the simplest physical expression of failure. The shape, although it might look right to some, was certainly not always right, for the air refused to cling to it.





FIGURES 1 and 2. An aerodynamic problem. Lift and (lift/speed²) plotted against speed, as the Mach number rises from 0 to 2·0. The angle of incidence of the wing is fixed.

There came a time when we were obliged to recognise that air is compressible. As von Kármán said in the discussion on my Wright Brothers Lecture in Washington in 1943, "It always was compressible"—but hitherto it had not generally mattered much. The times were changing.

To me, it was von Kármán's Wright Brothers Lecture of 1947 which made it clear that aeronautical engineers were facing an entirely new country—physically different from that in which they had gradually, and somewhat painfully, become at home. Moreover the change from one to the other was going to be difficult. It was again von Kármán who, at the First Anglo-American Conference in London in the same year, in what he himself described as an introduction to a discussion on high-speed stability and control problems, pointed out the unavoidable change in the aerodynamic behaviour of lifting surfaces and elongated bodies from subsonic to supersonic speeds, and the lack of our understanding of flow in the transition region where subsonic and supersonic flow patterns exist side by side at the same flight speed. He described the transonic speed range as the great unknown, and "an ideal field for the aerodynamicist practising his trade according to the definition: *he assumes everything but the responsibility.*"

We have many of us spent all the years since then in navigating the rapids which he saw so clearly ahead. The responsibility is something we had to shoulder. Ours is not the only sphere in which the coexistence of apparently incompatible ways of life has faced human beings with a problem to which there seems to be no solution free from continuous upheavals. The history of our own times must necessarily be lacking in perspective, even a mere diary. But we should at least attempt a narrative of what we have seen and done, before it is too late. The historian of the future, with the incomparable wisdom which comes to those who see everything in retrospect, will no doubt point out for the enlightenment of later generations both what we managed to achieve, and the errors of our ways. Well, we have one priceless advantage over him—we have had the fun as well as the burden.

The Transonic Problem

There is inevitably a lack of definiteness about the term "transonic speed range." It may stretch from as low as $M=0{\cdot}5$ to as high as $M=1{\cdot}5$, depending upon what we are dealing with. One thing about it has now been clear for some time—the most difficult part is that below $M=1$, the high subsonic range. I do not know who first talked about the "sound barrier," but this turned out to be little more than a milestone near the top of the hill, which we passed almost without noticing it. The hard going was the climb up to the top.

There is an interesting experiment which the Wright Brothers might have made, if only their wind tunnel had been able to reach high enough speeds. It consists of measuring the lift on a wing, at a fixed angle of incidence, varying the speed up to $M=2$ (Fig. 1). Their tunnel could reach only modest speeds, and had they tried to raise the speed by using more power, they would have succeeded only up to a certain point. The most striking feature of Fig. 1 would have remained unknown to them, since it was only a few years ago that John Stack, of the N.A.C.A., discovered how to make experiments over the whole range of speeds. In the wind tunnel there certainly was a sound barrier.

But in principle, the Wright Brothers might have been faced with the results shown, and it is interesting to consider what their reaction might have been.

One of the foundations of their outlook on aerodynamics was the idea that forces vary as the square of the speed. Although they did not explicitly accept this (JOURNAL, Royal Aeronautical Society, December 1953, page 759), it is clear that they believed it, and indeed regarded it as confirmed by some of their own experiments. So they would probably have replotted the results of Fig. 1 as a curve of (lift/speed²) against speed in order to see whether there were consistent discrepancies between their results and the V^2 law (Fig. 2). It is true that they were aiming at flying at a speed which is almost off the picture, to the left hand end of Fig. 1 (M much less than 0·1), so that to them most of the results would have been of academic interest only. But I am sure they would have regarded

the part of the speed range just below $M=1$, say from 0·75 to 0·95 as something to avoid. They would have probably felt reasonably happy up to, say, $M=0·5$, and content to leave higher speeds to the future.

The parabola in Fig. 1 and the horizontal line in Fig. 2 represent the V^2 relationship. There is an implication in this so-called "law" which is sometimes forgotten—that the *pattern* of the flow does not change with speed. So Fig. 2, which is the easier of the two to interpret, suggests that the flow pattern is substantially unchanged up to, say, half the speed of sound and then undergoes large and complex changes up to just below $M=1$. At higher Mach numbers it settles down to a comparatively orderly life, although it seems to change steadily and possibly continuously with Mach number.

I have always been surprised that relatively little effort seems to have been made in the early years of flight to examine the flow of air round models of wings or parts of aircraft. It is true that it has always been difficult to interpret flow pictures quantitatively. They are perhaps most valuable in retrospect, as a means of getting an insight into something we have observed in other ways.

We have now elegant ways of taking photographs of the flow of air with very short exposures, which show the detail, and help us to understand what is going on. Later, I propose to show a film which, as the speed changes, covers nearly the whole range of Mach number in Fig. 2. First, however, it will be useful to look at some stills, exploring the part where the changes are most striking, and have a direct relation to our experiences over the past ten years, when we have been navigating the rough water from $M=0·75$ upwards.

The results in Figs. 1 and 2 are typical of an ordinary wing at a small angle of incidence, when the flow at lowish speeds is smooth, and clings everywhere to the surface. Fig 3* is a photograph of "streamlines" round a wing at low speeds. At higher speeds, when the pressures produced change the density of the air considerably, it becomes possible and is more enlightening to use different techniques. There are three principal ones, which take advantage of the fact that light travels more slowly through dense air than through thin air; the one used in the following pictures is the "schlieren" method.

In Fig. 4† the angle of incidence of the wing is fixed at 2°, and the Mach number rises from 0·7 to 0·87. The photographs were taken with a very short exposure (about a millionth of a second), so that they show some detail which is only of secondary significance. The main point to remember is that the conspicuous lines (which

Courtesy of The Clarendon Press, Oxford
(See footnote to Fig. 3 in text)

FIGURE 3. Stream-line flow round a wing at a small angle of incidence and low Mach number. Note the boundary layer on the upper surface, and the absence of separation.

may be light, dark or coloured depending on the arrangement of the apparatus) in schlieren pictures, generally transverse to the direction of flow, indicate a density *gradient* and therefore a corresponding pressure gradient, which is large in the direction of flow. Such changes can occur only where the speed exceeds the speed of sound.

The first clear evidence of such a "shock wave" is at $M=0·8$ on the upper surface. It extends only a short distance in the cross-stream direction. It becomes more pronounced and longer as the Mach number rises, and at the highest Mach number (0·87) it is inclined backwards and extends far from the surface, outside the field of the photograph. Behind the wing there are signs of an increasingly wide "wake," in which the flow is turbulent. On the under-surface of the wing, a similar shock wave appears, but rather later.

The reason why the *local* speed exceeds the speed of sound, when the general speed of the air is subsonic, is that the mere presence of the wing causes increases of speed, which are greatest on the upper surface. This is an essential element in the mechanism of the production of a generally lower pressure on the upper surface than on the lower, *i.e.* a lift on the wing.

It is here that the main problem of high subsonic or transonic flight arises. The air finds no difficulty in accelerating to supersonic speeds. But it must eventually decelerate to the general subsonic speed of the flow, and this is something which it prefers to do rather suddenly, through the rise of pressure which a shock wave provides. There are known examples where the air decelerates from local speeds as high as $M=1·3$ without invoking the aid of a shock wave. But generally a shock wave is present, accompanied by a high adverse pressure gradient near to the surface of the wing and a premature separation of the flow from the surface.

This process is shown in more detail in Figs. 5-9, where the conditions at the five points marked *A*, *B*, *C*, *D* and *E* in Fig. 2 are displayed. In order to bring out the essential features of the flow, line drawings were made from schlieren photographs. The distribution of

*Figures 3 and 11 are reproduced through the courtesy of The Clarendon Press, Oxford, and are from Plate 12 (Figs. *b* and *c*) *Modern Developments in Fluid Dynamics*, Vol. I, edited by S. Goldstein, The Clarendon Press, Oxford, 1938. The photographs were originally provided by the late Professor Prandtl from a dissertation by Dr. F. Homann in *Forchung auf dem Gebiete Ingenieurwesens* 7, 1936.

†The photographs in this and the following figures are the work of the High Speed Group of the Aerodynamics Division, N.P.L., under Dr. Holder. To them, and to others who have helped me, my thanks are expressed at the end of this paper.

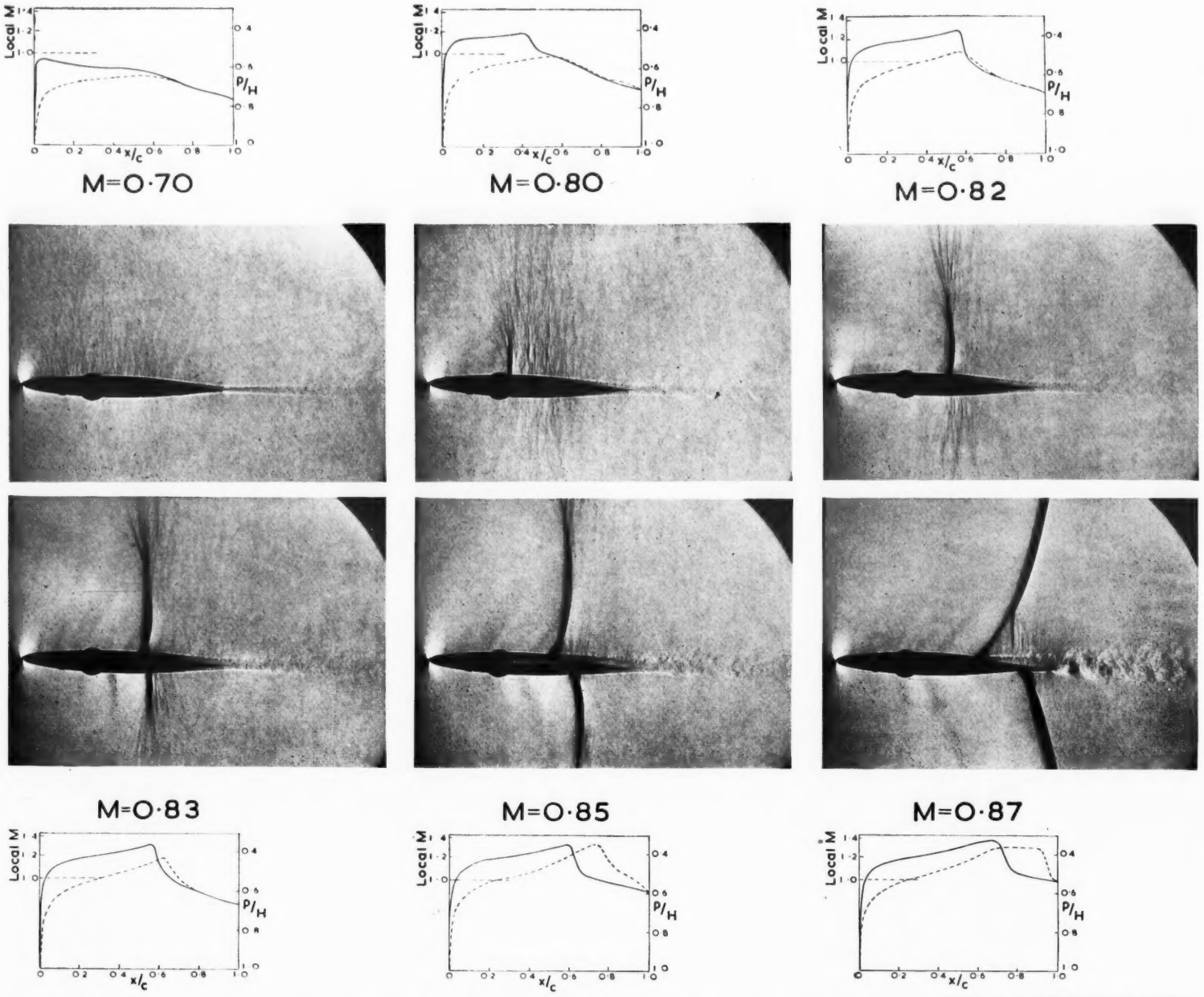


FIGURE 4. Flow round a wing at a fixed angle of incidence and high subsonic speeds. The diagrams show the pressure distribution on the surface and the local Mach number.

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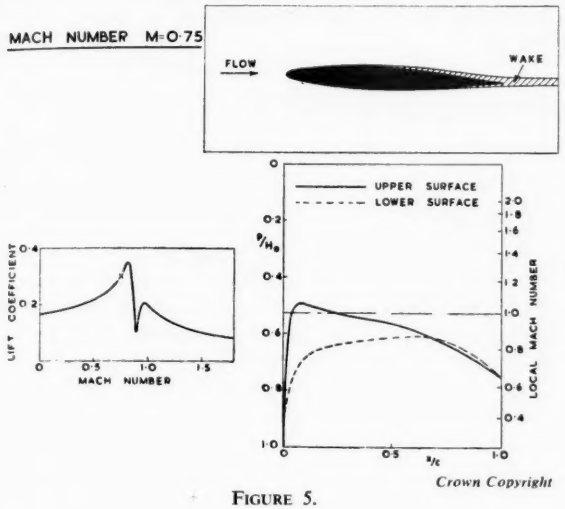

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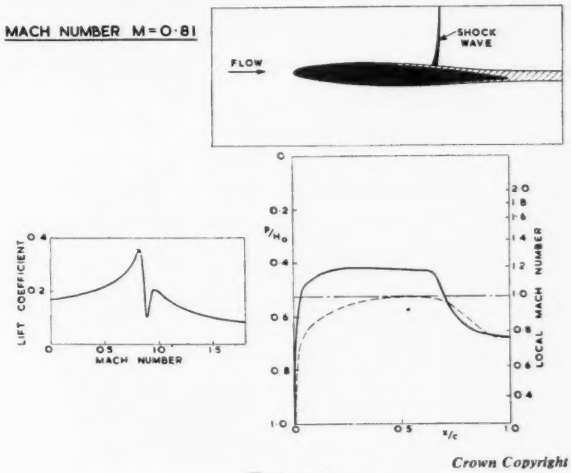

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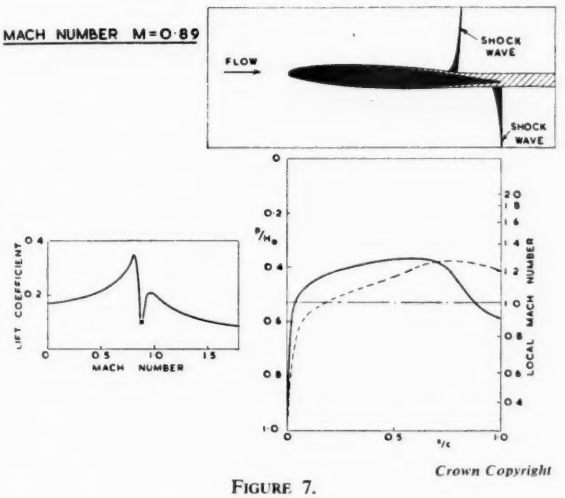

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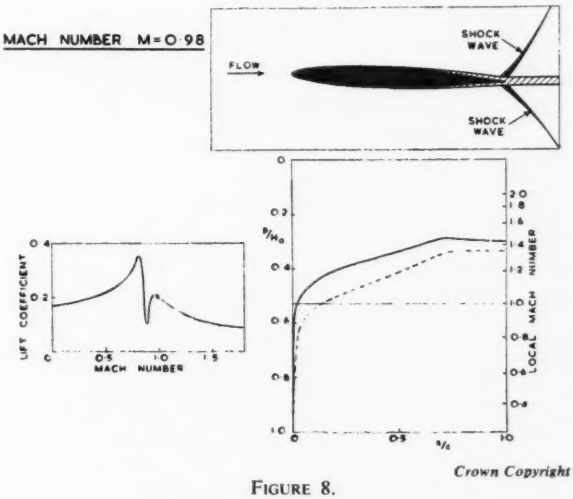

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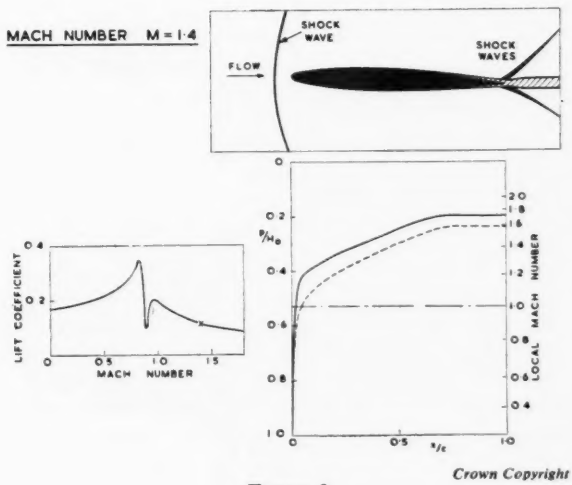

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FIGURE 9.

FIGURES 5 to 9. The transonic speed range. The changes in flow pattern, local Mach number, pressure distribution, and lift coefficient, at a fixed angle of incidence, as the Mach number rises from 0·75 to 1·4.

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FIGURE 4. Flow round a wing at a fixed angle of incidence and high subsonic speeds. The diagrams show the pressure distribution on the surface and the local Mach number.

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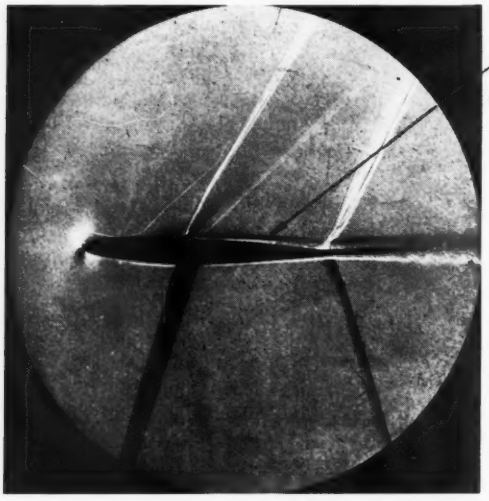

WITH GENERATORS

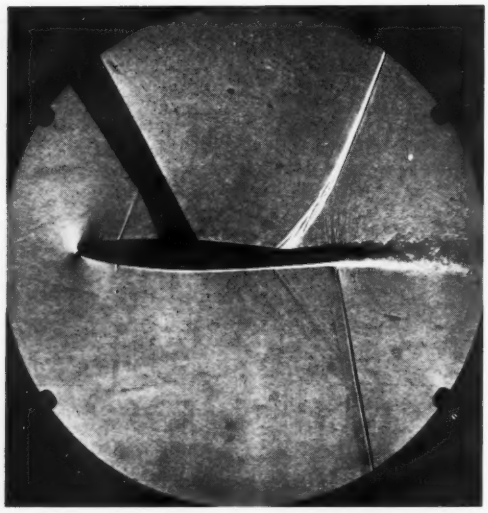

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FIGURE 10. The effect of vortex generators on boundary layer separation at a high Mach number. ($\alpha=2°$, $M=0{\cdot}93$.) The generators are on the upper surface, at about 40 per cent. of the chord from the leading edge. Note that the shock wave on the upper surface moves to the trailing edge and separation is prevented.

pressure round the wing was measured, and the corresponding lift coefficients calculated, equivalent to the (lift/speed^2) of Fig. 2. These figures enable a quantitative interpretation to be put on the flow pictures which were essentially like those of Fig. 4.

In Fig. 5, $M=0{\cdot}75$, the conditions are similar to those at low Mach numbers, except in two respects. From pressure measurements on the surface it is possible to calculate the speed near the surface. On the upper surface, for a length of about one-tenth of the chord not far behind the leading edge, the speed exceeds the speed of sound, but deceleration takes place without any shock wave. The lift coefficient is about 60 per cent. greater than at low speeds at the same angle. This increase is close to that predicted by the theory which we associate with the names of Prandtl and Glauert.

In Fig. 6, $M=0{\cdot}81$, we reach point *B* of Fig. 2, where the lift coefficient reaches its highest value, just double the low speed value. Over most of the first 70 per cent. of the upper surface the flow is supersonic, M being approximately 1·2. Then the pressure along the surface starts to rise rapidly, corresponding to the appearance in the free stream of the shock wave. The wake is appreciably wider than at *A*, corresponding to "separation" of the flow at the foot of the shock wave. On the under-surface the flow is just subsonic everywhere, and no shock wave exists.

The big change occurs between *B* and *C*, in Fig. 2, where the lift coefficient falls from twice to two-thirds of its low speed value, while the Mach number changes from 0·81 to 0·89 (Fig. 7). The striking feature of Fig. 7 is that while the upper surface conditions are not greatly different from those of Fig. 6, those on the lower surface have changed radically. Over nearly the whole chord the speed is supersonic, and deceleration to subsonic speed occurs through a shock wave at the trailing edge. This shock wave on the under-surface in effect isolates the upper from the lower surface, and the pressure near the trailing edge is different on the two sides, that on the upper being the higher. The lift is drastically reduced, and the centre of pressure, which up to the point *B* had been moving slowly backwards, moves suddenly forwards. Separation at the foot of the upper surface shock wave is more conspicuous and the turbulent wake is wide. The conditions are substantially those of the sixth picture in Fig. 4.

Beyond point *C* of Fig. 2, as the Mach number increases, the lift coefficient rises rapidly and reaches approximately the low speed value at a Mach number of 0·98 at point *D*, Fig. 8. Here the upper surface shock wave reaches the trailing edge, and the picture is almost symmetrical. The wake is narrower, and the upper and lower surface pressures differ by roughly the same amount over the whole chord, so that the centre of pressure has now moved back nearly to the mid-point of the chord. The typical low subsonic position can be deduced from the pressure diagram of Fig. 5, approximately at the quarter point. Between points *A* ($M=0{\cdot}75$) and *D* ($M=0{\cdot}98$) the centre of pressure has moved over a distance of about one-quarter of the chord of the wing, towards the rear, interrupted by a sudden move forward at *C*.

We have now reached practically the speed of sound, in the free stream, and at higher Mach numbers the conditions near the wing remain much the same. At *E*, where $M=1{\cdot}4$ (Fig. 9), the lift coefficient has fallen to half its value at *D*, or two-thirds of the low subsonic value, but without any sudden changes. The shock waves at the trailing edge remain, but they are set at finer angles. The most striking feature of Fig. 9, however, is the detached shock wave in front of the leading edge. The flow around the wing is supersonic every-

where except very near the rounded nose, behind the "bow wave." Through this wave no pressure changes due to the existence of the wing can pass.

The gradual reduction of the lift coefficient at still higher Mach numbers is again in good agreement with theoretical predictions.

It is interesting to reflect that if we had nothing but measurements of total lift to guide us and no flow pictures or pressure distributions, we might be tempted to discard the result at point *C* of Fig. 2 as probably unreliable, and to smooth over the gulf between *B* and *D*. It is apparent, however, that it is in the change from *B* to *C* and *C* back to *D* that the main high-subsonic problem lies, and that the key to it is the unwillingness of the shock wave on the upper surface to move back to the trailing edge.

Supersonic speeds appear first on the upper surface because the local speeds there are higher. The shock wave that then forms starts to move towards the trailing edge as the freestream speed is increased. It can, however, continue to do this only if the pressure rise through it continues to increase sufficiently to bridge the widening gap between the pressures in the upstream supersonic flow, which fall with distance along the chord, and those in the downstream subsonic flow, which rise with distance along the chord. At some stage in the rearwards movement of the shock wave, this pressure rise frequently becomes too great for the tired air in the boundary layer near the surface to negotiate, so that this layer is forced to separate from the surface. This separation changes the form of the shock wave in such a way that it restricts the pressure rise at the surface to a nearly constant value. Thus the condition for the continuous rearwards movement of the shock wave can no longer be met, and it halts at a certain chordwise position.

One way of testing the soundness of this description of the interaction between the effects of viscosity and compressibility—between boundary layer and shock wave—or between Reynolds number and Mach number, is to try to work on the boundary layer itself. We could try removing it, by sucking it into the surface. This produces the expected effect, but is not practically convenient. Or we could try speeding it up—putting energy into it.

One ingenious scheme injects a small amount of high speed air into the boundary layer well before the shock wave, using what are called "vortex generators." In Fig. 10 the lower photograph shows substantially the same situation as Fig. 7, the separation of the boundary layer on the upper surface behind the shock wave being clear. In the upper photograph, with a row of vortex generators placed at about one-third of the chord from the leading edge on the upper surface, the separation is virtually removed, and the upper surface shock wave moves to the trailing edge. This photograph shows also the small shock waves and the image of the vortices produced by the vortex generators themselves.

Up to this point, we have examined the situation at a small angle of incidence. At larger incidences, the effect is more obvious and may be more serious. In

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FIGURE 11. The low speed stall. The flow round a wing at a large angle of incidence and low Mach number. Separation on the upper surface near the leading edge.

Fig. 11*, we have the counterpart to Fig. 3—the low speed separation from near the nose of a wing on the upper surface, due to the inability of the boundary layer to survive the high adverse pressure gradient, the rapid rise of pressure which follows the creation of a region of very low pressure where the flow turns the corner. This is the old-fashioned "stall."

The counterpart to Fig. 4, where the incidence was fixed and the Mach number rose, is Fig. 12, where the Mach number is fixed at 0·75, and the incidence rises from 2° to 12°. Two things are clear. First, the effects of compressibility are generally similar in the two pictures, but now strong shock waves are produced by the progressively higher local supersonic speeds, due in turn to the progressively lower pressures over the upper surface as the angle increases and the lift rises. Second, at the larger angles the separation behind the shock wave is gross and there is a wide turbulent wake.

It is interesting to note that there is no apparent separation at or near the leading edge. Unlike the low speed separation shown in Fig. 11, the local supersonic flow near the nose seems to be able to turn the corner, which is sharper on the relatively thin wing of Fig. 12, than on the one shown in Fig. 11. Clearly, both the shape of the wing near the leading edge, and certain differences between subsonic and supersonic flow, have strong influences on the way the air behaves in such circumstances. In Fig. 13 is shown the effect of deliberately modifying the wing near the nose, in order to take advantage of this phenomenon. The Mach number here is 0·7 and the incidence varies from 5° to 9°. By thinning the shape near the leading edge, and so reducing the radius of curvature of the nose, and at the same time cambering the shape, the air has been persuaded to cling to the surface, and the occurrence of appreciable separation is delayed for nearly 3°.

In interpreting the pictures from Fig. 4 onwards, where the influence of compressibility on the detail of the local flow is the chief point of interest, it is necessary

*See footnote to Figure 3 (p. 433).

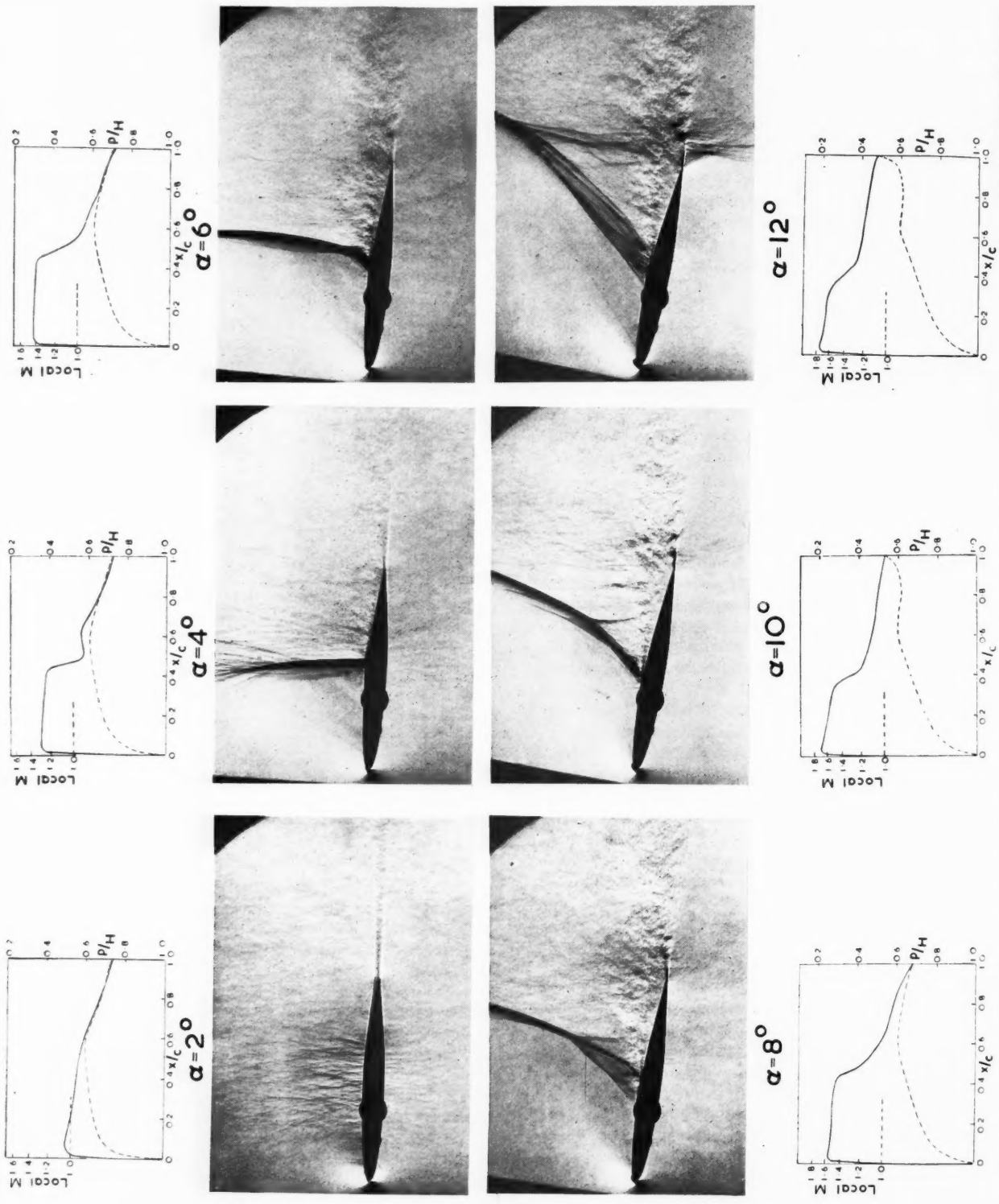

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FIGURE 12. Flow round a wing at a fixed subsonic Mach number (0·75) as the angle of incidence rises from 2° to 12°. The diagrams show the pressure distribution on the surface and the local Mach number.

FIGURE 12. Flow round a wing at a fixed subsonic Mach number (0·75) as the angle of incidence rises from 2° to 12°. The diagrams show the pressure distribution on the surface and the local Mach number.

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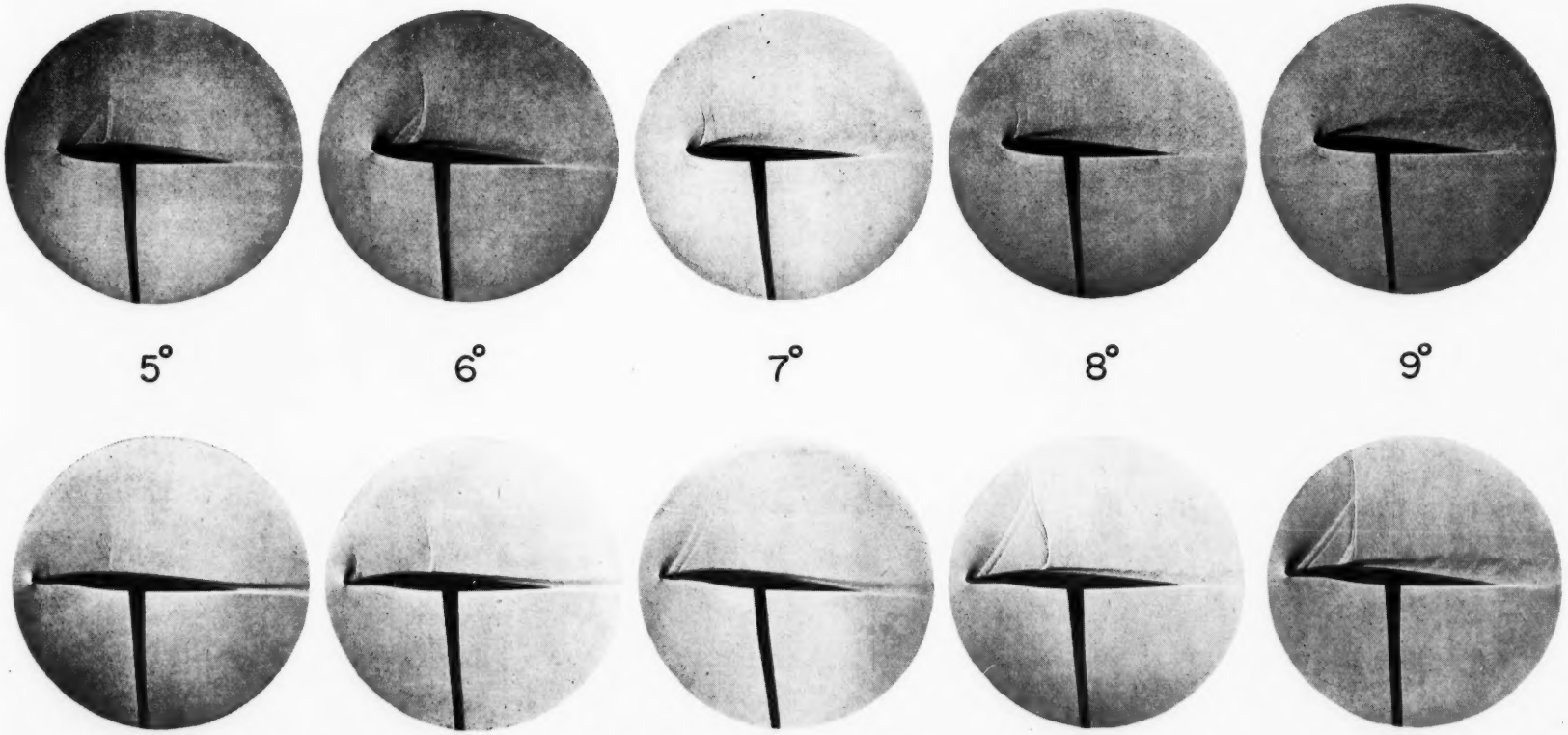

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FIGURE 13. The effect of a change in the shape of the forward part of a wing on boundary layer separation at Mach number 0·7. (Note that the first sign of separation is postponed by 3°, and leading edge separation is prevented.)

to remember that they are all pictures of two-dimensional flow. The model wing has the same cross-sectional shape and the same chord at every point of its span, and it spans the air stream at right angles to the direction of flow. Virtually all high subsonic or transonic aircraft have wings which are swept back. The essential idea behind sweepback is that the main local effects of compressibility are determined by the component of the speed of the air perpendicular to the *local* direction of the spanwise lines of equal pressure. If the wing is not tapered in plan (*i.e.* has a uniform chord across the span) these lines are approximately parallel to the leading or trailing edges. If the wing is tapered in plan, the local sweep is greatest at the leading edge and least at the trailing edge.

This is a qualitative description and not one which can be interpreted quantitatively in every case. But there is plenty of evidence that, in considering local rather than general effects, it is a good guide, though much more work must be done before we can expect to make quantitative inferences from two-dimensional pictures about three-dimensional conditions. Since, however, the angle of sweepback generally adopted is fairly large, an approximate allowance must be made for its effect. This is that the *flight* Mach numbers equivalent to the conditions shown, for example, in Fig. 4 would be considerably higher, possibly by as much as 40 per cent., than the Mach numbers attached to the pictures.

The laboratories and wind tunnels in which the work was done which produced the pictures with which this paper is illustrated are modest in size. Much of their equipment is of the same kind as will be found in a university, although the associated workshops and skilled craftsmen to make the models, without which the work could not be done, are much beyond what most university laboratories can afford.

We can have lots of such equipment and lots of people working at the problems in parallel. They can even work in different places, following different lines of approach, getting their own ideas, with freedom to follow them up. It is the sort of situation in which it is misleading to talk about duplication of effort. Indeed it is one where I think we should welcome what might be described as multiplication of effort. It was Rutherford who said, when somebody contrasted the poverty of our equipment with that of wealthier countries, " Well, we haven't got much money, so we've got to think." History suggests that we are not at all bad at thinking.

There are other techniques for looking at the flow, besides the schlieren used in most of the pictures I have shown. A rather striking one uses the optical interference between a beam of light that passes across the flow and a reference light beam. The result is a picture of interference fringes, familiar in other applications of the same principle, for instance to testing the curvature of surfaces of lenses. By this method, however, we have an approach to a quantitative outlook on the situation, since the air in each of the fringes shown has a constant density.

In Fig. 14 is an " interferometer " picture of a flow without separation. Figs. 15 and 16 contrast the flow round the same wing at 8° incidence at $M=0{\cdot}6$ and $M=0{\cdot}9$, by schlieren and interferometer pictures. At $M=0{\cdot}6$ there is a gross separation at the leading edge, and over the whole of the upper surface of the wing. At $M=0{\cdot}9$, the flow becomes supersonic round the extreme nose, the radiating fringes being the characteristic sign of a " supersonic expansion " by which, at supersonic speeds, air can turn round sharp corners. The conditions here are substantially those of the " Prandtl-Meyer " flow in truly supersonic flow. The ultimate return of the flow to the general subsonic speed through a complex series of shock waves, and the accompanying gross separation of the boundary layer, is clearly shown. Here it seems rather less formidable than in the schlieren picture. It is a good thing to have several points of view on a problem, but one needs to be an expert to make the best of them.

I have shown these last three pictures partly in order to lead up to the point that flow pictures are not necessarily merely qualitative, although their quantitative interpretation requires great experience. But I have another reason, namely to suggest that it would be far more satisfactory if we could so arrange the aircraft we make that their aerodynamics was simpler, so that these complex conditions never arose. It seems to me unlikely that we can ever be clever enough to live with conditions such as are represented in Figs. 15 and 16. After all, we look at Fig. 11—the old-fashioned " stall "—mainly to remind ourselves of the price we paid before we recognised that the only sensible thing to do about it was to steer clear of it. Why do we not take the same line about the " stall " associated with high Mach numbers?

The answer is that we do. But although in the end we came to understand the cause of the low speed stall, it is hardly true to say that we now design to avoid it, to go round it rather than through it. Nearly all aeroplanes can still be stalled. But they can be operated over the full range required by their lawful occasions without stalling, retaining a reasonable margin in the interests of safety. There is an art—a skill —required in the use, as well as in the design, of aircraft.

In the high subsonic speed range, however, we are faced with a rather different problem. The use of such schemes as vortex generators to inject energy—clean air—into the boundary layer, and a deliberate shaping of the front of the wing so as to use the ability of local supersonic flow to turn round a sharp corner without separation, may carry us far enough, so that by the use of rather thinner wings than those with which we first attacked the high subsonic speed range, we may expect to avoid most of the difficulties I have described. But it does not seem probable that high subsonic separation can be entirely excluded from all the conditions which a transonic aircraft has to meet, and it is necessary, therefore, to consider what is the harm which it might do.

The indirect harm has already been mentioned —large and undesirable changes in the pressure

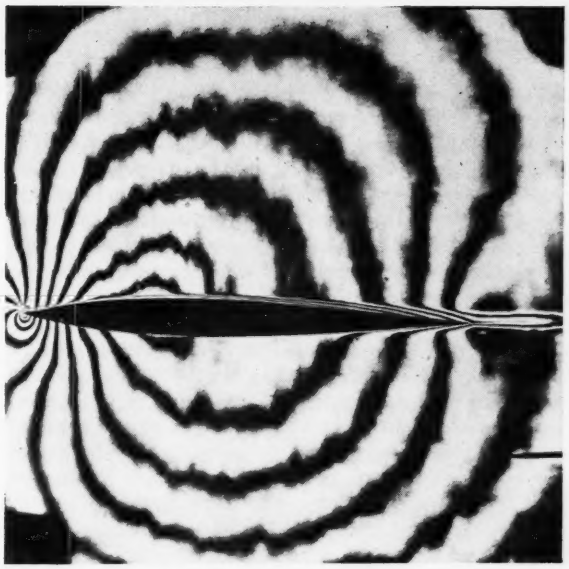
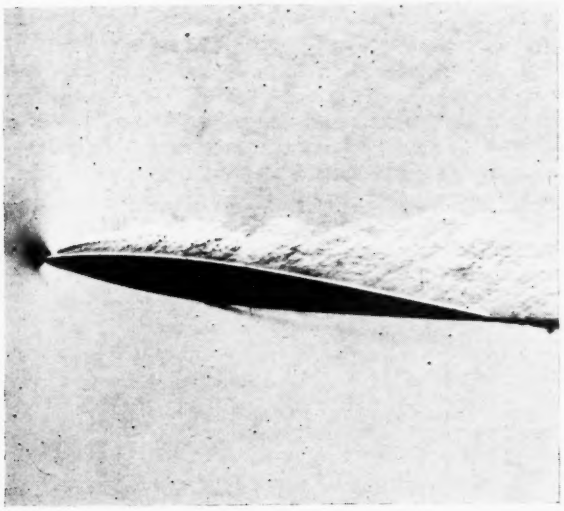
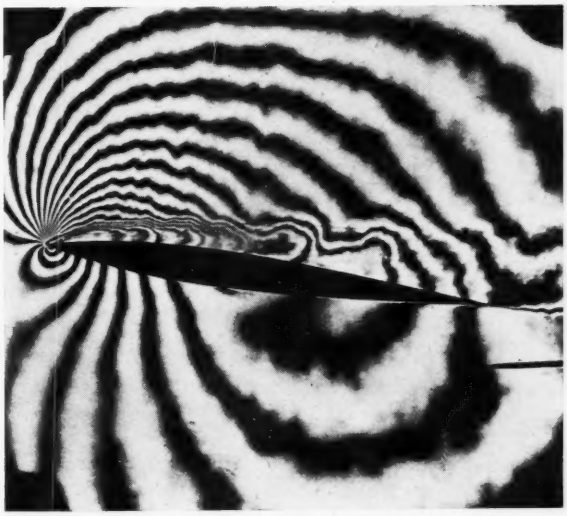
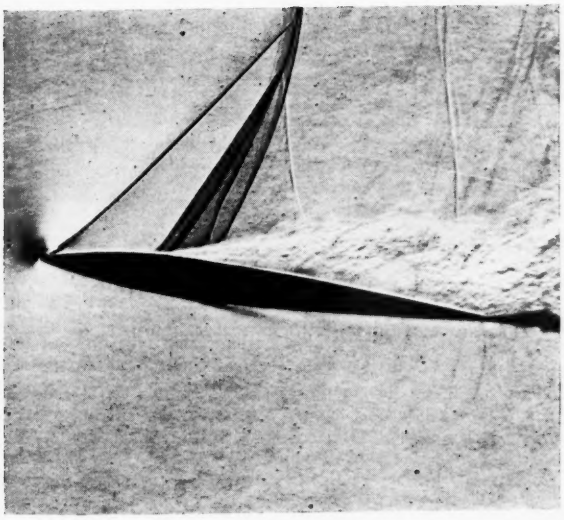


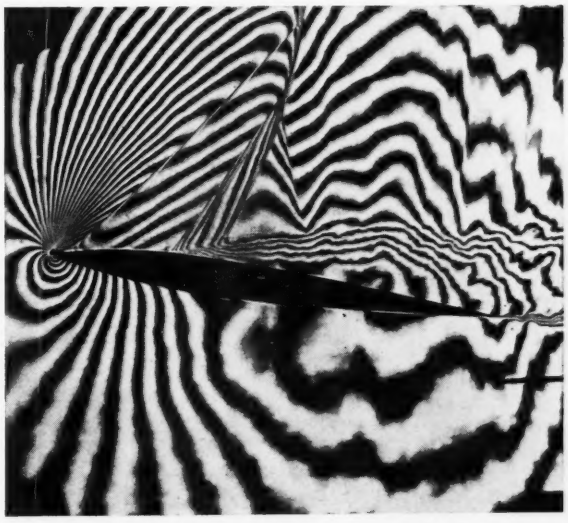
FIGURE 14 (*right*). Isobars. The flow round a wing, at a low Mach number and angle of incidence, shown by interference fringes. Boundary layer and wake, but no separation.

FIGURES 15 and 16 (*below*). Two points of view. Schlieren and interferometer pictures showing the effect of a rise of Mach number from 0·6 to 0·9 on separation at the leading edge. The angle of incidence is 8° throughout. The rise of Mach number has removed separation from the leading edge to the foot of a shock wave at about 30 per cent. of the chord.









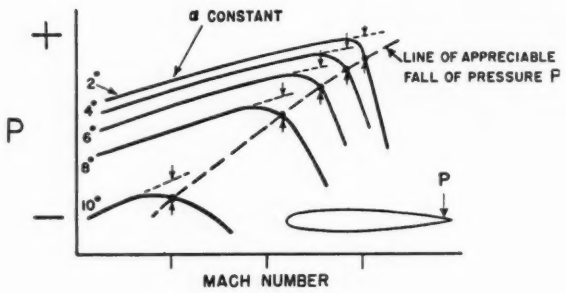
distribution and forces and, therefore, on the balance and control of the aircraft. The direct harm is vibration, leading possibly to fatigue of the structure. It is necessary to avoid exaggeration here, which might cause unwarranted alarm and despondency. Nearly everything vibrates, unfortunately, but obviously most things put up with it, and for a surprisingly long time. There have been big liners where sea-sickness was caused in smooth weather by vibration due primarily to the propellers, but so far as I know they did not fall to pieces. We travel across the Atlantic in aircraft which are full of vibrations, although we get used to them and tolerate them. But at very high speeds the variations of pressure in a separated flow can be fairly large, and since they repeat frequently, they act rather like a powerful pump going at a high speed. Separation can, as it were, pump energy into the structure near to it in the form of vibration. If we cannot eliminate it entirely, we must control it so that we are sure that there is an ample margin of safety.

It is here that I come to the second aspect of the work which has been done in the past few years in this field. We need to be able to relate the information which we can derive from the methods which I have described up to this point, to effects produced on the aircraft, in the form of fluctuating pressures and loads on its surfaces and structure.

Few problems have proved more difficult than the correlation between fluctuating air flow and its consequences to an aircraft. And the measurement of such fluctuations in wind tunnel experiments on models, while possible in principle, is tedious and of limited utility because of difficulties in simulating effects associated with vibration of the aircraft structure. Even the measurement in flight of fluctuating forces, while technically easier, because there is space in which to put the necessary equipment, is far from easy to interpret in terms of what can be accepted as tolerable and safe. It is much the same as the situation we face about noise, and probably for the same reason. We can measure it in several ways, but we find it very difficult to decide how much we can put up with.

In this field, however, valuable advances have been made recently by which we can link up observations of the flow in two dimensions with wind tunnel experiments on two-dimensional models of typical strips of a wing, and on three-dimensional models of the whole wing. These in turn can be related to the characteristics of the aircraft in flight. It has been found possible to cover the full range of angle of incidence (or lift coefficient, or the equivalent "g" on the aircraft) and of Mach number.

Every such attack needs a theory on which to base its plan of campaign. Many hard things are said about "theory" and "practice," but sensibly interpreted a theory is nothing but a working hypothesis. It was assumed that if the onset of separation could be detected, it would be possible to correlate it with the onset of fluctuating pressures on the aircraft and its consequences. Measurements of the pressure on the surface of model wings at constant angles of incidence and varying Mach numbers, showed that the onset of the effects of separation coincided with a marked tendency for the pressure at a point near the trailing



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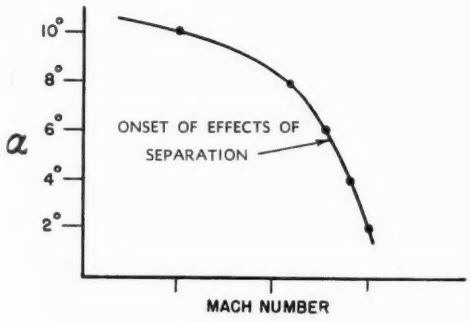

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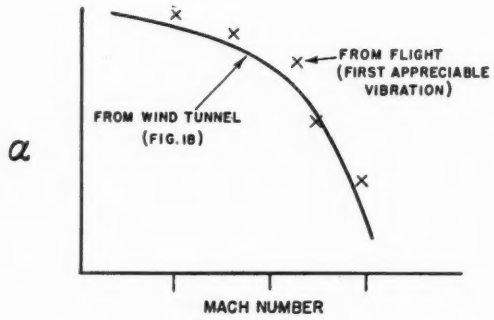


FIGURE 19.

FIGURES 17, 18 and 19. The evolution of a working hypothesis. A comparison between the first appreciable vibration on an aircraft in flight and of the onset of the effects of separation on a model in a wind tunnel.

edge, after rising steadily as the Mach number rose, to fall suddenly*. Fig. 17 shows qualitatively the general character of measurements of this pressure. The fall of pressure is not literally sudden, but making the usual sort of inspired guess, which is the key to most problems, that when the pressure has fallen by a small arbitrary amount below what the value would have been had it gone on increasing steadily, one can construct Fig. 18, a diagram showing the relation between angle of incidence and Mach number at which one might expect the effects of separation to be evident. These measurements could be made at a point along the span of a model where experience suggested that separation was likely to occur. They were compared with observations of the effects of fluctuating pressures in flight, using two different methods which themselves gave remarkably consistent results. These were measurements of vibration by accelerometers and observations by observers in the aircraft. The sort of correlation between these which has been obtained is shown in Fig. 19, which is about as good as one can reasonably expect.

How much vibration we are prepared to tolerate, or to accept as structurally safe, is a separate matter, but at least we have now a method of comparing laboratory work, and our physical understanding of the underlying flow conditions, with what we observe and feel in flight.

The flight instruments were put in places where, from other evidence, the effects of vibration were most distinct, and were adjusted to a high sensitivity, the object being to detect the first measurable sign of vibration. The human observers were experienced in detecting signs of very slight vibration, far less in intensity than what passes without comment in the majority of aircraft. The background level of vibration in the aircraft used in these experiments was very low. It is perhaps surprising, and certainly important, that the two methods of observation in flight gave points lying on a single curve. It seems, therefore, a fair conclusion that either method can be used to give a reliable indication of the existence of a level of vibration far less than one normally regards as a matter of concern. If we use the two together to determine what is acceptable on all grounds, we shall be on the safe side.

The fact that these flight observations agree with the curve derived from the wind tunnel experiments, which are in turn consistent with the observations of the effects of flow separation, means that we have now a logical method of attacking the problem, based upon firm foundations.

The next step was to try schemes which would be expected to reduce or eliminate separation, to test them on a model of the complete wing, and in flight. The results were remarkably consistent. There was reason to believe that we had separation " under control."

*I believe that the first suggestion that this characteristic of the trailing-edge pressure/Mach number relation would be a reliable indication of the onset of the effects of separation was made in 1954 in a paper by D. W. Holder, H. H. Pearcy and G. E. Gadd, on " The Interaction between Shock Waves and Boundary Layers."

It is at this point that I propose to leave this subject, but with this last word. It was not until the problem was met, and met in flight, that it was possible to assess how important it was. It was not a problem which could, in my opinion, conceivably have been attacked on purely general grounds, in the light of knowledge which we had when we started. There were signs that we should run into it, but it was impossible to say when, or how serious, it would be. As von Kármán said in 1947, " The transonic, the high subsonic region, remains the great unknown." We had to set off to explore the unknown, and we had no detailed map of it. We had plenty of warnings, but then we had plenty of warnings of other problems as well. If you set off on such a venture with nothing to help you but a list of warnings, you are quite likely never to start at all.

The Time it Takes

For the past few years we have heard on all hands that aircraft take an unconscionable time in coming out of the egg. Moreover, although they seem to fly reasonably well, they are not much good at anything else.

To those who complain that it should surely be possible to do this, that, or the other in less than so long, I am strongly tempted to say, " Come and try yourself." But I think it is more useful to point out that if you set out to explore unknown country, you must take risks and you must expect delays. The main thing is to get through to your objective. And if the time of arrival is what matters, start in good time, and don't hesitate or keep changing your mind.

New aircraft can arise in several ways. I am considering here one which is to be a substantial step forward. If we reckon time from the day when the need, the new requirement, first crystallises in the minds of those who have to plan for the future, and if we then look right forward, to the time of arrival in service of the first of a continuous stream of aircraft fit for use, it seems to be fairly general experience that about ten years will pass. How is all this time used up?

Perhaps a diagram (Fig. 20) may help to follow the argument. The horizontal scale is Time, the vertical is percentages of Effort, 100 per cent. being taken for convenience at some point of particular significance.

It seems to take about a year for those who do the planning to settle just what will be needed to meet the unforeseeable future. They must get all the skilled help they can on many problems, for they have to take ultimate responsibility.

It may take the best part of another year for the preliminary investigations of designers, the preparation of proposals and their review, and the reconsideration of the specification; for the revision of the chosen tenders, final agreement on a specification and the placing of orders to start work. Up to this point, about two years may pass.

Most of the next two years are occupied in getting the knowledge without which design, in the sense of making working drawings of details, cannot start, nor can special material be ordered. This is the time when the real foundations of the design must be firmly set.

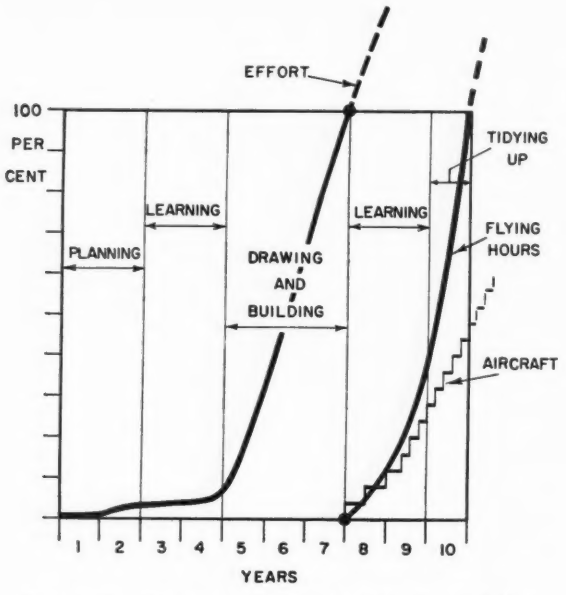


FIGURE 20. The time it takes—and what we do with it.

The more difficult the task, the more necessary it is to be satisfied that the final scheme is sound. It is not so much a question of working out one particular scheme in great detail, as of considering innumerable alternatives, and choosing between them, and finally weaving a pattern which seems to make the best compromise between all the requirements and problems. This may seem rather a long time to those who have not lived in this kind of world, but to those who are used to it, the time passes far too quickly. Even at the end of the two years it has to be recognised, by those who now bear the responsibility, that some problems remain which can be solved only when the aircraft flies. So far, it has taken four years.

It is now that what is called " detail design " begins. It is not always realised that in this stage designing and making go on simultaneously. The last drawing may be finished on the day when the first aircraft flies. It will take something like three years, so that we have now a total of about seven years from the start.

Up to this point, it may well be that only those who are concerned in the enterprise know anything about it. But from now onwards it becomes difficult to conceal that a new aircraft exists. Indeed, we seldom seem to try to conceal it. I say nothing about whether this is a good idea or not, but the consequences are not without their embarrassments. A new aircraft is exciting and controversial. It generally flies well, and the impression gets around that everything about it is perfect.

It is understandable that there is some mystification over the apparently slow progress in the next two years, or more, for it is now that the second period of getting knowledge begins. It demands not so much time in years as hours of flying. With all the ground tests which ingenuity can contrive, it is hours in the air which count now, and the time it will take depends mainly on the average number of aircraft that can be put into the air, and the resources that can be devoted to flying them. It may take as much as three years, and it can hardly be less than two, before the time comes when all qualities have been investigated, all deficiencies remedied, and usable aircraft emerge in a steady flow embodying everything that has been learned.

I am not concerned here when orders for these should be placed, but it does seem that the decision will have to be made at about the time that the first aircraft flies—before, rather than after—and therefore *before* what I may call the " flight " problems have even been discovered. It is during this period that the biggest strain is put on everyone concerned, and misunderstandings most readily arise. But there is no justification whatever for the suggestion that this situation is peculiar to the aircraft world, or to this country.

I want to emphasise that three, or possibly four, years out of the total of probably ten are spent in *getting knowledge*, and something like half this is concerned primarily with problems which originate from aerodynamics. And it is beyond doubt that it is during the second period of learning, after the first aircraft flies, when all the new ideas are for the first time put to the test of flight, that the shortage of experimental material (aircraft in flight) may be crippling to progress.

For the past ten years, the whole period since the end of the war, the problems of the transonic region have dominated our aerodynamics. The purpose of this paper has been to describe some of these and to explain how they have been attacked. I have thought it worth while to do this for several reasons. In the first place, the problems of the transonic region are intrinsically of great scientific interest, and very difficult. Our understanding of them has been advanced mainly by the kind of methods which I have described—what I call the aerodynamic art. Second, the main puzzle has been another form of the oldest problem of flying—stalling. How can we prevent separation of the flow from the surface of the wing, but now at very high speeds, under the effects of compressibility? Third, when we set out ten years or so ago to produce new aircraft which would have to live in this speed range, we knew that we should find ourselves up against problems of which we had virtually no experience. We should have to solve them when we had the aircraft in flight. Design would, therefore, have to move ahead of research.

I have been asked such questions as these. How is it that we have allowed this to happen? What is all this science for? What is the use of laboratories and wind tunnels? Why does research *always* lag behind design? Are we going on like this forever, always being late and producing aircraft which are obsolescent when they first fly and obsolete when they are first in service? To answer the last question first, all experience of similar enterprises shows that at the end of a venture which takes a fairly long time, we may expect to know several things which we did not know when we started. The first is whether our forecast of what would really be needed at the end of the venture was right or wrong, or in plainer English, " Did we guess right " ? Second.

knowledge having improved while we have been working, what is the right thing to do for the next venture? Third, where we might have done better. But fourth and most important of all, how to improve and to exploit the full potential of what we have just produced.

I think this really answers all the other questions. I sympathise with the underlying difficulty which prompted my friends to ask them. It is not so much that they were " blinded by science " as that they had not a sufficiently deep understanding either of the powers and the limitations of science, or of the methods it employs to discover knowledge.

What I have tried to show is that the processes of science are of many kinds. If we can really understand a field of knowledge, so that we arrive at a good working hypothesis—a theory—tested and proved by test to be reliable, then we are in a strong position. I mentioned, at the beginning of this paper, how, thirty years ago, the genius of a few men produced order out of chaos in aerodynamics, and made sense of our empirical gropings. But it was not so much to their mastery of the analytical processes involved that the impact of their work is due, as to the fact that they had also wonderful physical intuition and the inspiration to use it.

I see no reason to fear that we shall ever be short of such men. The very nature of what we do produces them. Nor is there any sign of a lack of problems.

The Changing Scene

I have shown you a film which draws together the threads I have tried to follow. It extends beyond the transonic region, and gives some idea of what we have to face in the truly supersonic field, which I define as starting at about $M=1{\cdot}5$. It tells nothing of the problems we faced in the earlier years, or how far we can truly say that we have solved them. We certainly succeeded in building good aircraft of many different sorts and sizes and shapes, not aerodynamic playthings, or oddities, but useful pieces of engineering.

Figure 21 is not one of those diagrams in which an effort is made to predict the future by plotting something against the date. It is simply a pictorial record of some typical aircraft, which I find it interesting to compare. The horizontal scale is of Mach number, and ranges from 0 to 2·0. All the aircraft are drawn to the same scale. I think I ought to make it clear that for all the outlines (with two exceptions) shown in Fig. 21 I am indebted to " Jane's All the World's Aircraft " or to " Interavia," and the lines which purport to represent what they can do are based entirely on information from the same sources. I hope that these two valuable publications will accept this acknowledgment of my debt to them. The two exceptions mentioned are the results of my own invention.

I do not think it is strange that I should take the Spitfire as a representative of the " Subsonic " days before we were seriously concerned with the compressibility of air. Its touch-down speed is the lowest shown on the chart and although it was not intended by design to exceed about $M=0{\cdot}65$, it was dived to over 90 per cent. of the speed of sound and survived unharmed. In all respects it was a wonderful aircraft over its whole speed range.

In the second group—" High Subsonic "—we first meet the problems I have described. I have taken three examples to emphasise the different ways in which engineers have used their art, sometimes puzzling the world by the diversity of their ideas.

I have included the Comet because I think it may fairly be described as the crystallisation of thirty years of experience, without venturing into the unknown, an example of enterprise and courage.

The Boeing B-47 and the Avro Vulcan are examples of two ways of meeting much the same requirement. They provide me with an opportunity to say that, if you have no room for the power plants inside, you must put them outside.

In the third group, " Transonic and Supersonic," are first the English Electric P1 and the Fairey Delta 2. When the first draft of this diagram was made, the solid line through the Fairey Delta 2 finished at about $M=1{\cdot}2$. It is good to know that it reached $M=1{\cdot}74$, 1,000 knots. These two aircraft may perhaps serve to illustrate a point which I made earlier in this paper, that once you have safely navigated the high subsonic region, there is smoother water ahead. It is mainly a question of pushing harder. This does not mean that there will be no other problems. There undoubtedly will be, and we shall have to deal with them when we meet them.

I was tempted to put the Lockheed F 104 in a group by itself, labelled " Supersonic only," which exposes the difficulty of trying to label things, particularly aircraft. It has a touch-down speed of about 150 knots and must therefore pass through the transonic region, but is probably intended to do so without hesitating.

Below it in the same group is an imaginary triangular shape, included mainly to emphasise that there is still plenty of room for variety of opinion about what shapes might make good aircraft. This third group contains examples of almost every shape that has been suggested so far, except the popular circle. This suggests that there is more than one way of doing this particular job.

Before I follow this further, I will mention the fourth group, which I have called " Subsonic *and* Supersonic." I have concocted this in order to emphasise what seems to me to be possibly the most important point in the whole diagram. The high speed of aircraft has risen in ten years from, say, 400 to 1,000 knots. We know that there are rocket-propelled aircraft which have gone even faster. This advance has, however, been bought largely at the cost of a rise in the take-off and touch-down speeds. The helicopter, the Fairey Gyrodyne, is an example of a flying machine which has an infinite speed range, defined as the ratio between top speed and touch-down speed. To the right of it is an imaginary shape, an enlargement of the one above it, definitely supersonic. The problem seems to me to be to combine the two, or rather to devise something which will have the qualities of both. To emphasise the difficulties of the

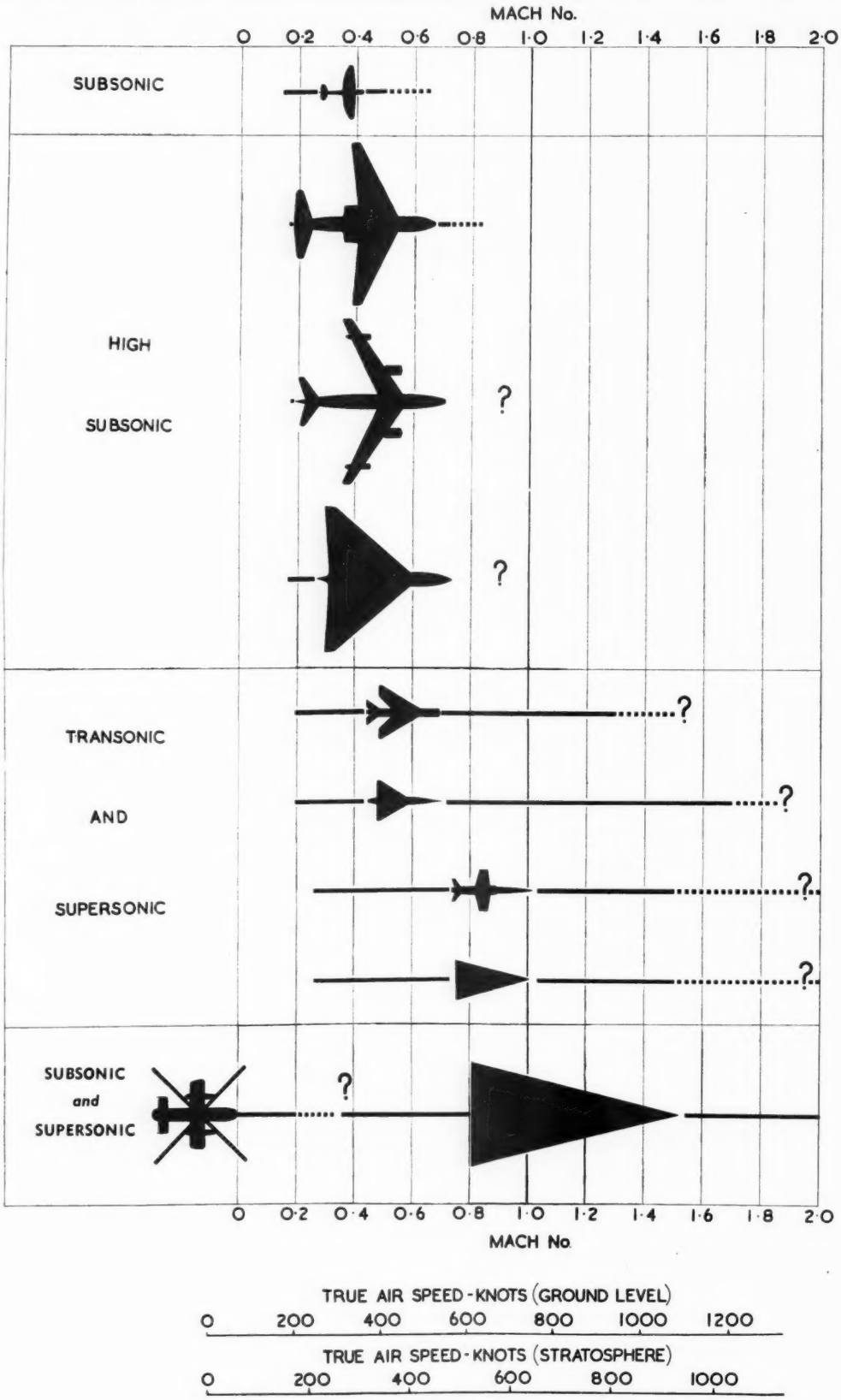
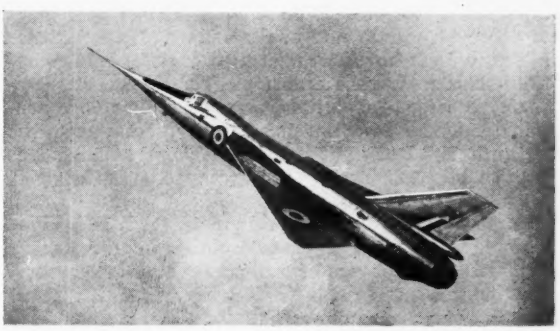


FIGURE 21. The changing scene—shapes in the sky.

problem, Fig. 22 shows photographs of the two Fairey products, the Gyrodyne and the Delta 2. Obviously we shall have to think of something better than a crude combination. But I do not believe we can expect to provide an acceptable solution to supersonic transport until we recognise that we have already gone farther than is wise in take-off and touch-down speeds. We certainly cannot afford to go farther in this direction merely in order to go faster.

To return in conclusion to the apparent inability of aircraft designers to make up their minds about what is the "right shape," or even the best shape, for high subsonic or supersonic aircraft, I want to take this opportunity to say that an aircraft exists to do useful work, and not to satisfy anyone's ideas of what looks right. You will remember how often we have had to recognise that what looked right a little while ago seems to look rather odd today. And of course, *vice versa*. But from what I have said about the nature of what is, after all, only *one* of the problems of this new world, it should not be surprising that it is the finer detail which matters, rather than the general outline. We shall see more, not fewer, shapes in the sky in future.

But it would be a dull world if all aircraft looked very much alike. I often feel cheered to notice how nature, to whom we sometimes appeal without much justification for help in such quandaries, has obviously been unable to make up her own mind. We may be thankful for the variety of life.



Courtesy of Charles E. Brown

The Fairey Delta 2

Courtesy of Charles E. Brown

The Fairey Gyrodyne

FIGURE 22. A problem of the future. Speed range—zero to 1,000 knots?

Acknowledgments

I am indebted to many friends for allowing me to use the results of their work in this paper. The Aerodynamics Division of the National Physical Laboratory, under Dr. W. P. Jones, and in particular the High Speed Group, under Dr. D. W. Holder, have not only provided the material for the flow pictures, but generously helped me to understand at least some of the finer points of them. I am grateful to Mr. H. H. Pearcy for much enlightenment. It is a pleasure to know that much of the work of this Group has been inspired by the experiences of aircraft in flight.

The interferometer photographs, Figs. 14 and 16, are from unpublished work by Mr. L. H. Tanner.

To the Aerodynamics Department of the Royal Aircraft Establishment are due many of the ideas upon which has been based the application of research on boundary layer effects at high Mach numbers to problems which have been met with aircraft at high subsonic speeds. I can only hope that I have not misrepresented them. If I do not mention anyone by name, it is because so many have contributed.

To the Shell Film Unit I owe the film which was taken specially for this lecture at the N.P.L. I am glad to know that it is one of a series which they are preparing, whose purpose is to enlighten us about the way air behaves. I can think of no better way of ensuring that we remember what we learn.

I acknowledge the courtesy of the Director of the National Physical Laboratory in consenting to the use of this material.

Finally, I am grateful to my friends and colleagues at A. V. Roe and Co. Ltd., Manchester, for the help they gave me in sorting out my ideas, which therefore now embody some of their own; and particularly to Mr. P. L. Sutcliffe.

VOTE OF THANKS

MR. E. T. JONES: Several of the previous 43 lectures are now classics and I do not think there is any doubt that the 44th will soon be one. However, there is nothing unusual in that. We expected it.

As you know we do not have a discussion after these lectures but I would like to call on Sir Arnold Hall to propose a vote of thanks to the Lecturer.

SIR ARNOLD HALL (*Fellow*): It is, of course, a difficult task to propose a vote of thanks when, by tradition, one is not allowed to discuss the lecture. I would not like you to think it is difficult because the lecturer has not done well; far from it. But even if it violates our own tradition, I would like to say one thing about what Sir William has said. I believe this lecture, and particularly the latter part of it, is very timely. I think there is a growing number

of people, rather than a diminishing number, who have not a complete grasp and understanding of what science seeks to do and how it is related to engineering achievement. Theodore von Kármán mentioned that the aerodynamicist is apt "to assume everything but the responsibility." Those who understand this process recognise that, properly used, it is one of the important methods which enable us to increase our understanding of complex phenomena but, if they are skilled in the art, they do not make the error of coming to believe that nature is really as simple as might be supposed by the assumptions they use to ease the problems of analysis. "To assume everything but the responsibility" is a legitimate artifice of scientific analysis; in contrast, it is far from reasonable to seek to assume authority, without a corresponding acceptance of responsibility, for making an engineering device work in the real world. Tonight's lecture has shown very clearly the differences between the two processes, and must provide anyone who has heard it, or who reads it, with a vivid picture not only of the relation between science and engineering achievement, but also of what it means to carry the responsibility for bringing an idea to the point of practical achievement. Let men of science by all means assist—as they do most outstandingly—by "assuming everything but responsibility" for the purpose of analysis, but let those who are skilled neither in this process nor in the bearing of the pain of practical development, see to it that they do not assume an authority in excess of its proper balance with responsibility. And that is the limit of my intrusion on our tradition of "no discussion."

As for the man, may I from my personal knowledge of him pay my own tribute to his greatness. I first met him when I was an undergraduate at Cambridge as did, I suppose, several of us in this room. I remember two things about him there. One was his incomparable teaching of fluid mechanics, for which many of us will be for ever grateful. The other was the occasion on 5th November when he was attacked in the middle of a lecture by a thing known in those days as a stink bomb—I expect that now it is called a stink device. His reply was to say with complete calm, "Gentlemen, I find it difficult to lecture in this atmosphere."

Then, of course, we remember him as Deputy Director of Scientific Research at the Air Ministry just before the war. I think if the post still existed it would now be called "Deputy Controller General of Science," or something of the kind, as there has been a certain inflation in these matters. Anyway, he was D.D.S.R., and we all owe him a very great debt for what he did at that time. Among the many things he did was one of particular importance; he brought to the fore the building of a wind tunnel in this country capable of speeds up to 600 miles an hour. This was in 1938. He was almost alone in bringing this about. The plant proved to be of exceptional importance in the development of aircraft during the war years, and if we had not possessed it, things would have gone less well than they did.

Then he became Director of Technical Development in the war-time Ministry of Aircraft Production, a post which our President now holds under a different title. When I think of him there, I remember the famous speech of Lord Beaverbrook on the radio. Beaverbrook spoke to the nation in the dark days of the war, and he was dealing with technical matters; he said: "Ask the boys in the back room; ask Farren." And so Sir William Farren became the first back-room-boy. I do not think my noble Lord realised how dangerous was his advice because if you ask Farren you are liable to be told!

But the most vivid tribute I have heard to his work at that time came when a distinguished war-time leader of the Royal Air Force, now dead, said to me, "His advice and determination at a time of tremendous pressure and technical difficulty were invaluable to us. No honour we could do him would equate to what he did for us."

He then became Director of the Royal Aircraft Establishment and I claim for myself a particularly good position to assess what he did there, first of all because I worked under him, and second because I was one of his successors. I believe the work he did at Farnborough during the war has always been under-assessed. That is not meant to be a criticism of anybody. It is just that what was done at that time was not widely known to the public, nor could it have been. I believe that his contribution to the war effort in the air was of tremendous importance, and I can certainly tell you this, that the seeds he sowed there of good organised scientific research still flourish in that great place.

Next he became a Technical Director in Industry, and so did I, so that I am a little restricted in what I can say about that particular breed of people. But I can say how delightful it is to be one of his colleagues. He is a tremendous source of inspiration and I know of no one more willing to give a helping hand. Technical Directors differ, of course, from Managing Directors; I have heard it said that you can always tell a Managing Director in the Aircraft Industry, but you can't tell him much!

I have mentioned but a few of Sir William's activities which spread so notably through the fields of science, engineering and education. He is a true follower of the Wrights. Let us remember that he had these things in common with the Wrights; he has pursued knowledge by research, he has designed, he has built, he has piloted. In the First World War he was well known as an outstanding designer-pilot; during the Second War, despite his heavy duties, he was still piloting Spitfires.

There is the background to the man. It is in your recognition of the great things he has done for British aviation for many years, as well as for the distinguished lecture he has delivered tonight, that I ask you to accord him your vote of thanks.

DR. E. S. MOULT (*Fellow*): I have known Sir William Farren for many years and I had the privilege of serving under him as a member of Council when he was President a year or more ago, and I can assure you all that the interests of this Society are very near and very dear to the heart of Sir William. It is therefore very fitting that he should have given us this paper tonight because we regard the Wilbur Wright Memorial Lecture as the most important paper of the year.

I came to this meeting tonight as a simple-minded engine man to learn something. Well, I have learned something and I go home not quite so simple-minded.

Some time ago, as engine people, we realised that aerodynamics concerned the flow through the engine as well as the flow around the engine, in fact I think we have now reached the stage where it is very difficult to distinguish between the two flows. Similarly, of course, as Sir William has told us, the aircraft designer has become more appreciative of the science of thermodynamics and he will even concede that the engine man has some good tricks in

mechanical engineering and the knowledge of metallurgy which can be applied to his structures at the high speeds that we are contemplating.

One foresees the need for an even closer co-ordination of effort between the aeroplane designer and propulsion engineer if we are to extend the range of speed of future aircraft as outlined in this lecture.

Sir William, we are all very grateful to you for this inspiring address and it gives me the greatest pleasure to second the vote of thanks so ably put by Sir Arnold Hall.

Following the Lecture the Annual Council Dinner was given by the President and Council of the Society at 4 Hamilton Place, W.1, at which the following were present:—

Dr. A. M. Ballantyne, T.D., B.Sc. (Eng.), Hon.F.C.A.I., A.F.I.A.S., F.R.Ae.S., Secretary, The Royal Aeronautical Society; Professor A. D. Baxter, M.Eng., M.I.Mech.E., F.R.Ae.S., Professor of Aircraft Propulsion, College of Aeronautics, Cranfield, Member of Council; Rear Admiral A. N. C. Bingley, O.B.E., Fifth Sea Lord; Mr. E. C. Bowyer, C.B.E., Director, The Society of British Aircraft Constructors; Major G. P. Bulman, C.B.E., B.Sc., F.R.Ae.S., Director of Construction of Research Facilities, Ministry of Supply, Honorary Treasurer of the Society; Air Marshal Sir Dermot A. Boyle, K.C.V.O., K.B.E., C.B., A.F.C., Chief of the Air Staff; Colonel George F. Brown, U.S.A.F., Research and Development Staff Officer, Defense Department, U.S.A.

Sir Sydney Camm, C.B.E., F.R.Ae.S., Director and Chief Designer, Hawker Aircraft Ltd., Member of Council and Past President; Colonel Paul A. Campbell, Senior Assistant Air Attaché, American Embassy; Dr. W. Cawood, C.B., C.B.E., B.Sc., Director-General of Scientific Research (Air), Ministry of Supply; Professor A. R. Collar, M.A., D.Sc., A.F.I.A.S., F.R.Ae.S., Professor of Aeronautical Engineering, Bristol University, Member of Council.

Mr. Handel Davies, M.Sc., A.F.I.A.S., F.R.Ae.S., Scientific Adviser to the Air Ministry, Member of Council; Mr. K. V. Diprose, B.A., awarded the Simms Gold Medal for 1956; Sir Roy H. Dobson, C.B.E., J.P., Hon.F.R.Ae.S., Honorary Fellow 1956; Dr. Hugh L. Dryden, Hon.F.I.A.S., Hon.F.R.Ae.S., Honorary Fellow 1956.

Mr. G. R. Edwards, C.B.E., B.Sc., F.R.Ae.S., Managing-Director, Vickers-Armstrongs (Aircraft) Ltd., President-Elect.

Sir William Farren, C.B., M.B.E., F.R.S., M.I.Mech.E., Hon.F.I.A.S., F.R.Ae.S., 1956 Wilbur Wright Memorial Lecturer, awarded the Society's Gold Medal for 1956, Past-President; Mr. J. Fenwick, A.M.I.E.E., awarded the British Silver Medal for 1956.

Mr. G. W. H. Gardner, C.B., C.B.E., B.Sc., F.R.Ae.S., Director, Royal Aircraft Establishment; Mr. H. H. Gardner, B.Sc., F.R.Ae.S., Chief Designer (Guided Weapons), Vickers-Armstrongs (Aircraft) Ltd., Member of Council.

Sir Arnold A. Hall, M.A., F.R.S., F.R.Ae.S., Technical Director, Hawker Siddeley Group, Member of Council; Mr. P. A. Hearne, D.C.Ae., D.L.C., Grad.R.Ae.S., Helicopter Project Engineer, British European Airways, Member of Council.

Sir Bennett Melvill Jones, C.B.E., F.R.S., Hon.F.I.A.S., Hon.F.R.Ae.S.; Mr. E. T. Jones, C.B., O.B.E., M.Eng., F.R.Ae.S., President, The Royal Aeronautical Society, Director-General of Technical Development (Air), Ministry of Supply; Sir Owen Wansbrough-Jones, K.B.E., C.B., Ph.D., Chief Scientist, Ministry of Supply; Dr. W. P. Jones, M.A., D.Sc., A.F.I.A.S., F.R.Ae.S., Superintendent of Aerodynamics Division, National Physical Laboratory.

Dr. T. von Kármán, Hon.F.I.A.S., Hon.F.R.Ae.S., Chairman, A.G.A.R.D.

Mr. B. P. Laight, M.Sc., A.M.I.Mech.E., F.R.Ae.S., Chief Designer (Aircraft), Blackburn and General Aircraft Ltd., Member of Council.

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Mr. F. M. Owner, C.B.E., M.Sc.(Tech.), M.I.Mech.E., F.R.Ae.S., Deputy Chief Engineer, de Havilland Engine Co. Ltd., Member of Council.

Mr. J. J. Parkes, F.R.Ae.S., President, The Society of British Aircraft Constructors; Colonel R. L. Preston, C.B.E., A.F.R.Ae.S., Secretary-General, Royal Aero Club.

Mr. L. Eugene Root, Member Research and Development Technical Advisory Panel on Aeronautics, U.S.A.; Captain J. A. Ruddy, Acting U.S. Naval Attaché and Acting Naval Attaché for Air.

Mr. J. E. Serby, C.B.E., F.R.Ae.S., Director-General Guided Weapons, Ministry of Supply; Sir Reginald Verdon-Smith, M.A., B.C.L., J.P., Joint Managing Director, Bristol Aeroplane Co. Ltd.; Mr. John Stack, Assistant Director of Langley Memorial Laboratory and Consultant to the Office of the Secretary of Defense (Research and Development) U.S.A.; Group Captain W. K. Stewart, C.B.E., A.F.C., B.Sc., M.B., Ch.B., awarded the Wakefield Gold Medal for 1956.

Dr. P. B. Walker, C.B.E., M.A., F.R.Ae.S., Head of Structures Department, R.A.E., Member of Council; Dr. F. L. Wattendorf, F.R.Ae.S., Director of the Secretariat, A.G.A.R.D.; Mr. L. A. Wingfield, M.C., D.F.C., A.R.Ae.S., Solicitor to The Royal Aeronautical Society; Mr. R. H. Woodall, M.I.E.E., F.R.Ae.S., awarded the George Taylor (Australia) Gold Medal for 1956.

International Airworthiness Standards

by

SIR FREDERICK TYMMS, K.C.I.E., M.C., F.R.Ae.S.

(United Kingdom Representative on the Council of I.C.A.O. from 1947-1954)

FROM THE EARLIEST DAYS, it has been an accepted article of faith that every aircraft flying outside its own country should carry some guarantee of its safety, conforming to an international standard, if not controlled internationally. No aim has been more assiduously pursued. But, after nearly forty years of the international regulation of aviation, there is no international standard of airworthiness with which aircraft are compelled to comply. The questions that now arise are whether the objective is ever likely to be achieved, and whether, in fact, international standards are necessary to safety.

The draft convention considered by the first inter-State conference on international aviation, in Paris, in 1910—a draft whose preparation had started ten years earlier—required that an aircraft should have a "certificate of navigability," issued or authenticated by the State whose nationality it possessed. The title suggests that authorities were then concerned more with the simple question whether the machine could fly than with the intricate tests which now determine an aircraft's airworthiness. It was in fact argued that it was impossible in the state of knowledge of the day, and with the rapid development taking place, to certify that a flying machine was safe. That may well have been. However, the requirement remained a part of the draft code which provided the foundation for the Paris Convention of 1919.

The Paris Convention 1919

The Paris Convention took it one step farther. First, it required that every aircraft engaged in international navigation should have a certificate of airworthiness issued or rendered valid by the State whose nationality it possesses, and placed an obligation on the other contracting States to recognise such certificate as valid—by inference, valid for the purpose of flight over their territory. Secondly, in Annex B to the Convention, it laid down the main conditions which should govern the issue of certificates of airworthiness by States, and provided that the minimum requirements to be fulfilled would be fixed by the International Commission for Air Navigation. Thereby, the Convention committed the States parties thereto to the principle of international standards of airworthiness.

1919 was more informed than 1913. In place of single aeroplanes here and there, scores of thousands had been designed, built and flown in Europe. There was no longer any doubt about the possibility of controlling airworthiness—nationally at least; it was a necessity. Internationally, it was possible to write into Annex B only the barest skeleton of the conditions which should be fulfilled, based on the much more elaborate machinery and procedures which each producing country had built up for military purposes. Thus, it was required that the design of the aircraft should conform to certain minimum requirements; each type of aircraft must satisfactorily demonstrate its flying qualities in flight trials conforming with certain minimum requirements (but subsequent aircraft conforming with the approved type were not required to undergo such trials); and the workmanship and materials in every aircraft must be approved under a system of control of construction conforming with certain minimum requirements. Pending the formulation of the international minimum requirements for each of these conditions, each State was required to determine detailed regulations under which its certificates of airworthiness would be issued or remain valid.

Supplementing Annex B (which, being a part of the Convention, was binding on contracting States), Annex C required every aircraft engaged in international navigation to be provided with aircraft and engine log books, maintaining a complete engineering record of the aircraft and engines, including all tests, overhauls, replacements and repairs. Thereby some control of the maintenance of airworthiness was implied.

Since these provisions of the Convention applied to all aircraft flying internationally, the motive was clearly the protection of third parties in other countries, although they incidentally served to protect the crews and any passengers and goods that might be carried.

The I.C.A.N.* Minimum Requirements for Airworthiness

It was not until fifteen years later that the International Commission produced the first regulations concerning the minimum requirements and, although called regulations, they were not in fact made part of

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*International Commission for Air Navigation.

the Convention and had not the binding effect of an Annex. This adoption by Resolution gave them in fact the status of recommendations. Up to the outbreak of the Second World War, therefore, while the I.C.A.N. States were under an obligation, in principle, to conform with international standards, there were no international standards. (It should be understood that the term "international standard" is used, throughout, in the sense of an obligation imposed on States by international agreement, as opposed to a recommendation). Except for the interchange of information and ideas, each State established its own standards. The régime under which aircraft flew freely over the territory of other contracting States was based on the acceptance by those States of the certificate of the State of Registry that the aircraft conformed with the national standards of airworthiness of that State.

Other Pre-War Conventions

Outside the I.C.A.N. States nothing had happened to set up a different international pattern. Either there were no agreements or those that existed established the same situation. The Pan-American (Havana) Convention of 1928, which had no technical annexes, did not contemplate the establishment of international standards. Under that Convention, aircraft were required to carry a certificate that, in the opinion of the authorities of the State of Registry, the aircraft complied with the airworthiness requirements of each State whose territory would be flown over. Nevertheless, the right was reserved to each State to refuse to recognise such a certificate as valid for further flight over its territory if, on inspection by a duly authorised commission of that State, the aircraft was found not to be reasonably airworthy in accordance with the normal requirements of that State. It was no doubt contemplated that the "duly authorised commission" would only be set in motion in case of some flagrant attempt to fly an aircraft which appeared clearly unsafe to fly. It was, no doubt, regarded as an ultimate safeguard rather than as a piece of machinery for normal use. In fact, it is believed never to have been used and, so far as aircraft did fly between the United States and the Latin American countries, their national certificates of airworthiness were accepted. Since these aircraft were almost wholly American and certified as complying with United States airworthiness requirements, it was not difficult to certify that they complied with any other national requirements that might be in force. The system, although it might have been suitable to the peculiar circumstances of the case, would not appear to offer much of value for present use.

Scope of the I.C.A.N. Regulations

The I.C.A.N. "Regulations concerning the Minimum Requirements for Airworthiness Certificates" are of importance here only as indicating the intentions of the States. They were comprehensive in scope, even if meagre in content compared with the more modern codes of I.C.A.O. (International Civil Aviation Organisation). They covered aeroplanes—both landplanes and seaplanes—and gliders; but they were not developed for airships or rotary wing aircraft. With separate provisions for type and subsequent aircraft, the Regulations prescribed standards for performance in take-off and landing, handling qualities, structural strength, engines, equipment, material, inspection and tests. They were, in fact, a compendium in outline of the principles and practices which had been found good in national practice up to then. The Regulations were made applicable from 1st January 1936 to all new types of aeroplanes of the normal category to be used in international air navigation, and in respect of which a certificate of airworthiness was applied for on or after that date. The impossibility of making the Regulations retroactive in effect was recognised, but not provided for to the full extent that I.C.A.O. found necessary later.

Aims of the Chicago Convention

The régime that the States assembled at Chicago in 1944 set out to establish through I.C.A.O. was considerably more rigorous than any that had been attempted before the war. While providing in the Convention that the Council should adopt "international standards and recommended practices" on airworthiness, and while such international standards impose an obligation on the States, this was not deemed to afford sufficient security for the airworthiness of aircraft or the qualifications of air crew personnel. Article 33* provides that the obligation to recognise certificates of airworthiness issued by the State of Registry shall be dependent on the aircraft satisfying the minimum standards established pursuant to the Convention—at least from the time such minimum standards have been established. There is disagreement as to the intention and the meaning of Article 33; and, despite a Resolution† of the Assembly in 1948, directing the Council to study it and, if necessary, initiate an amendment of the Article, the Council and the States have neglected to take any further action.

There is fairly general agreement that the purpose for which Article 33 requires the recognition of certificates is to facilitate the flight of aircraft over the territory of other States. The context could hardly, in fact, admit of any other interpretation. There is disagreement as to whether Article 33 obliges a contracting State to recognise the certificate of airworthiness of an aircraft issued prior to the establishment of I.C.A.O. standards applicable to that aircraft. The intention of the provision was to afford, as had the Paris Convention, a basis on which international navigation could proceed during this period, for which a considerable duration could be foreseen. States for whom practical considerations weigh heaviest accept the intention. Those who

*_Article 33._ Certificates of airworthiness and certificates of competency and licences issued or rendered valid by the contracting State in which the aircraft is registered shall be recognised as valid by the other contracting States, provided that the requirements under which such certificates and licences were issued or rendered valid are equal to or above the minimum standards which may be established from time to time pursuant to this Convention.

†Resolution A2-44. I.C.A.O. Doc. 6736—C/775.

prefer a strictly legal interpretation of the words see in them no obligation. Whether regarded as an obligation or not, contracting States do, in fact, as the Assembly Resolution urged, recognise the certificates of airworthiness issued by other States to their own registered aircraft. There is no case on record of an aircraft being refused passage over another country, because of the inadequacy of the standards on which its certificate of airworthiness was issued.

What is not in doubt in the interpretation of Article 33 is that, under the I.C.A.O. régime, it is intended that there shall be international standards, and that compliance with these, alone, shall secure for an aircraft to which they are applicable the right of flight over the territory of other contracting States.

Certification for Import and Export

There is no attempt in the Convention to attach to a contracting State any obligation to accept a certificate issued by another State, whether based on national standards or international standards, as the basis for the issue of its own certificate of airworthiness. Article 40* of the Convention seems to indicate that such an obligation was at one time contemplated, since it specifically provides that the registration or use of an aircraft whose certificate of airworthiness bears an endorsement of differences from international standards shall be at the discretion of the State into which it is imported. As there is no obligation to accept, for this purpose, a certificate of conformity with international standards, the provision serves no useful purpose. Each State is free to impose its own standards, however rigorous, on imported aircraft—or to impose none at all! In practice, the country with a well-developed manufacturing industry imposes rigorous conditions on imported aircraft; the less developed country is content to accept the standards of the exporting country.

Some few bilateral agreements have been made to define the conditions on which each party will accept the certificate of airworthiness of the other for the purpose of certification on its register. Such an agreement was first made between the United Kingdom and the United States in 1932. It does not follow that such agreements are operative to secure automatic acceptance by each party of the airworthiness certification of the other.

The Airworthiness Annex (Annex 8) incorporates a provision which appears to have a close bearing. A State issuing its own certificate of airworthiness to an imported aircraft, is permitted to accept the certificate of airworthiness issued by another contracting State, in lieu of carrying out its own investigation, inspection and test. In the context of the Annex and the Convention, however, the real significance of this can only be to ensure that the validity for international flight of such certificates of airworthiness shall not be challenged and, secondarily, to permit a State to certificate an aircraft in an I.C.A.O. category, although not itself the primary authority.

At one time it appeared desirable to rectify the omission—if it can be regarded as such—in the Convention. On the proposal of the United States, the Fourth Session of the Assembly in 1950 set in motion an examination of the possibility of securing international agreement on a single set of airworthiness standards for the recognition of certificates of airworthiness for the export and import of aircraft. There was considerable reluctance on the part of States to accept any restriction on their freedom of action in regard to the certification of aircraft built in other countries and, after further examination by the Air Navigation Commission and Council, the proposal was dropped. The fact that it was then thought possible to secure agreement on this reflected a more sanguine view of the future of international airworthiness standards, than has been justified by subsequent progress on Annex 8, to serve the simpler purpose of the Convention.

The Penal Clauses of the Convention

That no rights of international navigation are intended to be given by the Convention, except for aircraft conforming to international standards of airworthiness, is emphasised by Articles 39 and 40*. The first requires that every aircraft which fails in any respect to satisfy an international standard of airworthiness shall have details of the variation endorsed on its certificate of airworthiness (a more practical method of implementing the intention has since been agreed); and the second provides that any such aircraft shall only participate in international navigation with the permission of the State whose territory is crossed. Whatever freedom of flight is granted by or under Articles 5 or 6 of the Convention† is thus withheld from aircraft which do not conform to international airworthiness standards.

Delayed Application of Airworthiness Standards

The Convention recognises that many airworthiness specifications are of such a character that they can only

*_Article 39._ (_a_) Any aircraft or part therof with respect to which there exists an international standard of airworthiness or performance, and which failed in any respect to satisfy that standard at the time of its certification, shall have endorsed on or attached to its airworthiness certificate a complete enumeration of the details in respect of which it so failed.
Article 40. No aircraft or personnel having certificates or licences so endorsed shall participate in international navigation, except with the permission of the State or States whose territory is entered. The registration or use of any such aircraft, or of any certificated aircraft part, in any State other than that in which it was originally certificated shall be at the discretion of the State into which the aircraft or part is imported.

†_Article 5_ defines the rights accorded by the Convention and the associated conditions for the flight of aircraft, other than those engaged in international scheduled air services, over the territories of contracting States. It covers both private aircraft and aircraft engaged in air transport or other activities. _Article 6_ makes it clear that the right to operate a scheduled international air service must be sought outside the Convention, by special permission or authorisation of the State whose territory is entered. The text of these articles is not important for the present purpose.

be introduced, if at all, at an early stage in the design of an aircraft; and Article 41* gives three years' grace. By virtue of this, the provisions of Chapter VI† of the Convention do not apply to aircraft types submitted for certification before the expiry of three years from the date of adoption of an international standard. (It is now questionable whether three years is long enough). While the intention of Article 41 has been the subject of dispute, the practical interpretation is that aircraft that have passed the drawing board stage are saved from the prohibitory provisions of Article 40. The important converse follows—that, so far as any mandatory effect of the Convention is concerned, international standards of airworthiness can only be aimed at future generations of aircraft. They cannot, whatever their nature, be imposed on aircraft flying now or in the near future. When the existing generation of aircraft types has been retired—perhaps twenty-five years hence—any international standards now in force may apply to all aircraft flying. It is a fair deduction that, whatever the object, I.C.A.O.'s ten years of work on this subject is not aimed at present safety.

The Airworthiness Annex

Annex 8 (Airworthiness of Aircraft) contains the standards and recommended practices adopted by the Council and put into force in pursuance of the Convention. First adopted in 1949, it has been expanded in several stages and consists of three parts, of which Parts II and III are significant.

Part II of the Annex may be regarded as the modern and much enlarged counterpart of Annex B of the Paris Convention. It contains the principles, methods and procedures essential to a system of airworthiness control, starting with the design and construction of an aircraft and ending with its day to day maintenance. The whole of this material has been given the status of international standards. The bar of Article 41 was deemed not to apply to administrative standards and they were brought into force on 1st October 1949, for application as early as practicable in the circumstances of each case. That part of the Annex, therefore, gives effect to standards in the way contemplated by the Convention.

Part III of Annex 8 contains standards for two I.C.A.O. categories of transport aircraft*. Category A is designed to embrace the most advanced types of multi-engined aeroplanes; the specifications apply to aeroplanes with two or more engines and demand a high standard of take-off and climb performance with one engine inoperative. Category D (introduced in 1950) is designed for aeroplanes of limited weight (not exceeding 12,500 lb.), having a low stalling speed (not exceeding 56 m.p.h.). These two categories do not purport to cover the whole range of aeroplane types to be used in international air transport. Pending completion, the standards, although formally adopted in accordance with the Convention and imposing the obligation of Article 38 to notify differences in national regulations, in fact only apply if a State elects to certify an aeroplane in one of the I.C.A.O. categories. This is entirely optional and, in fact, few types of aeroplanes have been so certificated.

The Development of the Problem in I.C.A.O.

To appreciate the problem, as it now presents itself, it is necessary to look briefly back over the course that has been run. Starting from the basis of the draft Annex to the Convention, produced at Chicago, the first session of the Airworthiness Division, in 1946, produced draft international standards for Category A aeroplanes. They extended to the airframe, engines (piston engines only), propellers, sundry accessories and equipment. They covered design, construction and testing; flight loads and structural strength; and flight characteristics, including performance of the aeroplane. At this early stage, the main structure of what is in Annex 8 was prepared. These standards were intended to be supplemented, as regards performance and equipment, by operational standards, which would be dependent on route and aerodrome characteristics. Three things of significance have to be noted in the recommendations of the First Airworthiness Division. Firstly, the standards evolved for Transport Category A aeroplanes were intended to be enforced for all aeroplanes engaged in scheduled transport of passengers. It was the intention later to produce standards for aeroplanes used in non-scheduled passenger transport, aeroplanes used exclusively for freight carrying and for other classes of aircraft. Thus, aeroplanes that could not, for example, conform with the "one engine inoperative" performance rule, would be debarred from use in scheduled

*_Article 41._ The provisions of this Chapter shall not apply to aircraft and aircraft equipment of types of which the prototype is submitted to the appropriate national authorities for certification prior to a date three years after the date of adoption of an international standard of airworthiness for such equipment.

†_Chapter VI_ of the Convention, comprising Articles 37 to 42, contains the provisions under which the Council of I.C.A.O. adopts international standards and recommended practices on a variety of subjects (Article 37) and the conditions attaching to them. Article 38 requires a State that is unable to comply in all respects with an international standard, to give notice to I.C.A.O. of the differences between its national and the international practice, and requires I.C.A.O. to publish the information. Articles 39(*a*), 40 and 41 contain the penal and escape provisions for aircraft; these are textually quoted separately. Articles 39(*b*) and 42 relate only to personnel licences.

*The I.C.A.O. categories of aeroplanes cannot be defined except by reference to the complete set of standards for each category. The objective is that the categories shall together cover all the aeroplanes of varying performance characteristics likely to be used in air transport. Thus Category A is intended to accommodate the large, high-speed aeroplanes of today and tomorow; Category D is for small highly manoeuvrable aeroplanes more akin to those of the past. Categories B and C were proposed at different times to span the gap between the two extremes—Category B for aeroplanes which might just fail to meet the conditions of Category A; and Category C for aeroplanes somewhat larger and with a higher stalling speed than those defined in Category D. These two categories have not been established and it is possible that neither will be.

passenger transport. Later (1950) another category (D) was developed for these.

Secondly, the Division, while recognising that the date of enforcement must be sufficiently far ahead not to penalise current types of aeroplanes, nevertheless thought it necessary to plan specific dates of enforcement to ensure progressive application of the standards. They contemplated a more immediate future than has since been found acceptable or in accordance with the Convention. They proposed to apply the standards within five years (January 1951) to all individual aeroplanes newly introduced into service thereafter, and within eight years to every aeroplane then engaged in scheduled international passenger transport. They were prepared to see the Convention amended, if necessary, to make this possible. The Council later found that amendment would be necessary and did not recommend it.

Thirdly, the Division did not consider it necessary to enforce international standards of airworthiness on aircraft engaged in private flying, thus revealing that the Division was more concerned with the risk to persons and property in an aeroplane than with the risk of damage to third parties on the surface. It was not observed that this distinction also, in due course, would necessitate amendment of the Convention.

The second session of the Division, in 1947, devoted itself to the improvement of the draft standards and recommended practices for Transport Category A aeroplanes, and began to wrestle with some of the problems which, later, caused so much difficulty. It was recognised that scheduled passenger transport flying could not be confined to aeroplanes conforming to Category A. Other categories would have to be provided for aeroplanes which, while unable to comply with the take-off and climb specifications with one engine inoperative, yet had another factor of safety, in a lower stalling speed.

The Division endorsed its previous recommendation that the standards now drafted should be brought into force on fixed dates in the proximate future, and recommended that Article 41 should be expunged from the Convention. So soon, however, as it was admitted that the standards available could not be applied to all the types of aeroplanes which might be produced for scheduled passenger transport, the first recommendation began to lose its force. The door was open for optional compliance with standards—a contradiction in terms—and the long trail started towards that distant goal of a complete range of standards, which could be given the force provided for in the Convention.

Important issues concerned with the performance of aircraft as an element of airworthiness came to the fore. The need for basic performance specifications, as distinct from operational performance standards depending on aerodrome and air route conditions, was questioned. The problem of temperature accountability, to smooth out the uneconomic and the unsafe margins of performance inherent in the existing formulae, in varying atmospheric conditions, was opened up. Inequities resulting from the use of rate of climb formulae designed for aircraft in the medium speed bracket, and not suitable for the newer aircraft with high stalling speeds, constituted another problem. These were among the issues which underlay a substantial part of future work. In the outcome, the standards of performance eventually adopted were concerned mainly with the method of determining and scheduling performance—an essential for the application of operational standards—while quantitative elements were stated as recommendations.

A special committee of the Airworthiness and Operations Divisions was convened in September 1947, to deal with the problem of temperature accountability. The third session of the Division followed in the Spring of 1949, and put on record a massive achievement in the development of standards and recommended practices and preparation for the solution of specific problems, but began to show signs that the limit of agreement would fall short of the ultimate goal. Standards were worked out for Transport Categories C and D, but symptomatic was the succession of almost equally divided votes on the former—for example, on weight limits, limitation of weather conditions, and even its necessity. While the standards which were evolved could not be enforced, except on aeroplanes not yet even planned, there was some indication that thought was influenced by the limitations of existing aeroplanes. No clear concept had emerged, much less been agreed, as to what categories would be needed before international standards would be available for all aircraft to be used in international navigation, or for any particular purpose. The development of standards for a Cargo Category of transport aeroplane was agreed as a future project, only I.A.T.A. (International Air Transport Association) holding the view that cargo aeroplanes should conform with passenger aeroplane standards—another reflection of the different aims of third party protection and protection of the occupants. Performance requirements began to play a major rôle and progressively became more complex. A special performance committee was proposed to deal with the British proposal for rational performance standards, that is standards based on the probability of the performance of an aeroplane falling below the prescribed datum. A large programme of collection of data was recommended, for both the temperature accountability and the rational performance studies.

In the meantime, the first edition of the Airworthiness Annex, containing standards and recommended practices for Category A transport aeroplanes, had been adopted by the Council in March 1949. In June 1950, the Council adopted standards for Category D transport aeroplanes, to come into force in February 1951. The uncertain recommendations for Category C were rejected. The Annex drew attention to the fact that it was not mandatory to certificate an aeroplane in either Category A or Category D, and that standards for other categories would be produced later. In effect, Article 41, which the Assembly had, in 1948, refused to remove from the Convention, was by-passed.

So long as the alternative of not conforming with an international standard is available, there is no demand for protection from its early imposition.

The subject of performance was pursued at meetings of the Airworthiness and Operations Divisions in 1951 and by a standing committee on performance. The fourth session of the Air Division, apart from this, developed existing standards, especially for the installation of turbine engines, to complete those for the testing of turbine engines already in the Annex. These were adopted and incorporated in the third edition of the Annex.

This session produced one of the classic disagreements which have dogged this subject—when and how could an aeroplane be certificated in an I.C.A.O. category? It revealed the second of the fundamental difficulties in attaining that state of development which would permit international standards to be enforced. The United Kingdom, pointing out the inevitability, in a progressive world, of new aeroplane types embodying features which had not yet found their way into I.C.A.O. standards—turbine engines were a current case in point—considered that it should be possible to certify such aeroplanes in an I.C.A.O. category, if they conformed with the standards already in force; otherwise it was thought that an I.C.A.O. certificate would become a "certificate of obsolescence." The United States, having regard to the fact that some of these features would have a significant bearing on safety, believed it inadmissible to certificate an aeroplane embodying them in an I.C.A.O. category. While the aeroplane might be certified as conforming with such I.C.A.O. standards as exist, the right of entry for such aeroplanes must be dealt with on a reciprocal basis. Whatever privileges Article 33 might confer, therefore, would not extend to aeroplanes in advance of I.C.A.O. standards. The only solution envisaged was to complete the I.C.A.O. airworthiness standards as soon as possible and, in the meantime, to refrain from certificating aeroplanes in an I.C.A.O. category. A famous debate terminated by the Council recommending to States that they should refrain from certificating aeroplanes in an I.C.A.O. category (in spite of the standards adopted for that purpose) until the standards were complete or until the Council had come to decisions on basic policy. While the major gap in the standards that served to bring the difference to a head resulted from failure to reach agreement on performance specifications, it was not disputed that development would be continuous and would continue to create new gaps between the standards and current practice that was ahead of them.

Realisation of the difficulty of achieving a state of completion in the formulation of international standards and of reaching agreement on precise, quantitative specifications, if they were to have mandatory effect, led to a change in the direction of attack, and new machinery was devised. It was accepted in principle that the Annex should be rewritten to incorporate only broad qualitative standards, and a special Airworthiness Panel was set up to do the redrafting—composed of experts from States who had wide experience of the problem and who were prepared to devote some of their resources and time to it. Meanwhile, the Standing Committee on Performance continued its examination of the new methods of specification. The Panel reached the conclusion, at an early stage, that a broad specification of standards was inadequate to define a level of airworthiness, and it was conceived that the standards should be supplemented by illustrations of "acceptable means of compliance." This, the latest phase of the work, has extended over some three or four years since its first conception. Drafts of the new standards and their complementary "acceptable means of compliance" have indeed been prepared. Agreement has not yet been reached on the principles of performance specification. The real test of whether any advance has been made towards a solution lies in the status to be given (or which is given by the Convention) to the international standards and the acceptable means of compliance.

The Convention, in fact, forces contradictory conclusions. Without question, only international standards, constitutionally adopted, can impose any obligation on the States. Neither recommendations nor "acceptable means of compliance," however phrased, can do so. Yet the technical finding is that broad standards cannot define a level of airworthiness. If the failure to agree on precisely specified standards is accepted, this is tantamount to saying that the desired level of airworthiness, even of the future generations of aircraft (which are the only aim) cannot be imposed by international obligation. Attempts to mitigate this effect and ensure future uniformity, by insisting that there shall be only one acceptable means of compliance associated by I.C.A.O. with any standard, in effect, give a false value to something which is not authorised by the Convention. Apart from this, that approach tends to reimpose the inflexibility and restriction of future design, which it was the object of the "broad" standards to escape. The advantages of international uniformity, manifest in such matters as air traffic control and maps, are not necessarily the over-riding consideration in airworthiness.

The restriction of Article 41 leads to another line of attack—or escape! In order to impose some international obligations deemed necessary for aircraft currently flying, it is proposed to segregate from the Airworthiness Annex, and include as standards in other Annexes, all those matters which can be regarded, somehow, as something other than airworthiness, thus escaping the embargos of Articles 39, 40 and 41. This means an extension, to the utmost practicable limit, of the method already adopted of dealing with standards of performance, fuel reserves and equipment in Annex 6 (Operations). This is logical and, in fact, inescapable in respect of the changeable elements in the make-up of an aircraft—the elements that are capable of adjustment to the conditions of a particular route or flight.

No one would contemplate that there should be no international standards governing, for example, the weight and performance of an aircraft in relation to the aerodromes it is permitted to use. No one would deny

that those standards should apply to aircraft flying now. The fact is that they are not standards of airworthiness, and the danger is that the attempt to escape the hampering clauses of the Convention affecting airworthiness standards and the certificate of airworthiness may distort the result. It must be noted, however, that the value of these operational standards, as an exercise in international regulation, can only be realised if all States refrain loyally from exercising their right to impose on the aircraft of other States—as some have done—their own higher standards of performance, equipment, fuel reserves or whatever else they may please.

Failure to Achieve the Objective of the Convention

The general objective of safety in international civil aviation is the keynote of the Convention. The aim, so far as the airworthiness of aircraft is concerned, as expressed in Article 33 and Chapter VI, is to achieve that safety by imposing on all aircraft flying internationally an obligation to conform with agreed international standards of airworthiness. The particular object of the States can be deduced—it is to protect the lives and property of their citizens, as well as passengers, mails and goods, from injury due to defects in foreign aircraft flying over their territory. In thirty-five years of international flying, that protection, so far as it has been afforded, has not derived from international standards. The "safe and orderly growth of international civil aviation"—to quote the Convention—has not been hampered by the lack of such standards. States have been content, whether they consider themselves bound by Article 33 or not, to accept, in good faith, the certificates of other contracting States in respect of their aircraft. The responsible authorities of all countries have endeavoured to live up to this trust, by emulating the standards of the leading countries in aircraft production and operation, and particularly those of the country of manufacture of the aircraft. They have drawn such guidance as they could from the preparatory work on international standards. Insurance requirements also have operated to maintain the level of safety. But, the compelling power has been the national will and commercial competition—not the force of an international obligation.

The delaying effect of Article 41 removes to the distant future any possibility of an obligatory international standard of airworthiness contributing to the safety of international air navigation. There is no question of the necessity to exempt existing types of aircraft from the application of most—but not necessarily all—international standards of airworthiness. The aircraft cannot be rebuilt, nor can they be scrapped. However, the maintenance of a compulsory condition of international flight, without regard to the fundamental obstacles to its implementation, is illusory. A high level of international standards, compulsorily applied to aircraft yet to be built, may play its part in "improving the breed," but that is not the object for which obligations were written into the Convention; nor will international obligations be a major factor in producing that future improvement. It may be that the compulsion of international standards, applicable only to future aircraft, will eventually contribute to safety in international air navigation; but the safety of the great-grand-children of the men who first conceived that international airworthiness standards were necessary for safety can hardly be regarded as a satisfactory accomplishment. If the concept is right, it must be more speedily realisable.

If it were possible to conceive of the complete and final formulation, within some reasonable period of time, of the international standards for all classes of aircraft, then it might be possible to accept some further delay before they became effective and still feel that the pursuit of the original objective had been justified. It is not possible. The unending evolution of aircraft design will continue to leave as wide a gap as ever between the standards that have been developed and the new types of aircraft coming into being. At any moment of time, therefore, there will be new aircraft flying which do not conform to the current international standards; they may embody features not covered by the standards, or they may be of a class not even contemplated by the standards. The assurance of safety by international enforcement can only, at best, be partial. The safety of the newer generation of aircraft will, as now, depend on the national standards of airworthiness imposed, in the first place, by the country of manufacture and, in the second place, by the country of import.

What the Work of I.C.A.O. has Achieved

Because the specific object of the Convention has not been achieved and, it appears, cannot be achieved, it must not be supposed that all that has been done has served no purpose. On the contrary, it has been of value to all countries, from the foremost aircraft producer to the smallest user. All that has been done can be said to serve the general principles of the Convention and the aims and objectives of the Organisation as set out in the Convention. The bending of the effort, however, to serve the particular object of the binding and penal clauses of Article 33 and Chapter VI has both hindered and warped the achievement.

The Airworthiness Annex has served as a model for the framing of national airworthiness regulations. The British Civil Airworthiness Regulations have been revised to give effect to the provisions of Annex 8, so far as they have been developed and are applicable. Countries which are not producers of aircraft and have not had the same incentive and opportunity to develop their own codes of regulations, have been provided with a model from which to draw. Particularly is this true of Part II of the Annex, which consists of enduring principles, which may be applied without delay to all aircraft.

Importers of aircraft have used the international standards and recommended practices of Annex 8 as the basis for purchase specifications, thus forcing the

manufacturing countries—both constructors and certifying authorities—to conform. Being bilateral transactions, these have not been hampered by the lag of international standards behind technical development. National standards can be grafted where the I.C.A.O. standards are incomplete, and alternative methods of achieving the same level of safety can be accepted by the parties concerned. While I.C.A.O. abandoned the attempt to establish international standards for the recognition of certificates of airworthiness for export and import of aircraft, the standards developed to serve as a condition for international flight have been borrowed and used in bilateral arrangements for this purpose. What would not be acceptable as the basis of a multilateral agreement imposing an obligation on the parties is readily adapted to more flexible bilateral arrangements. Nevertheless, the enforced concentration on future generations of aircraft and the sustained opposition to alternative methods of achieving the same level of safety give grounds for thinking that the prevailing motive in the formulation of I.C.A.O. standards has been the value of an I.C.A.O. hallmark for the purchase and sale of aircraft, more than the object prescribed in the Convention.

Not least among the valuable results of the prolonged I.C.A.O. effort to establish international standards has been the continued interchange of ideas and views between the authorities and technicians of many countries, and particularly those with large scale manufacturing and operating industries. In a rapidly and continually developing science and technique, this cannot fail to influence future progress, as well as to educate the authorities—administrative and technical—in the control of airworthiness in their own countries. By many, this would be regarded as the most valuable product of the effort, and good reason for continuing it. It cannot be usefully continued without the stimulus of an end to be achieved.

An Advisory International Code

All these ends can be served without pursuing the will o' the wisp goal which has proved impossible to reach. An international advisory code of airworthiness, established and continually developed by international agreement, without the obligation attaching to international standards under the Convention, would have this effect. The production of such a code would not be beset by the obstacles that have so far retarded progress. The impossibility of accepting the bar to progress of an international commitment to enforce standards of a precise detailed character has led to the change to broad qualitative standards. Without the commitment, the dilemma of choosing the right course between standards too rigidly framed and standards too broad to have any real significance would disappear. An advisory code of airworthiness would be framed in as much detail as necessary to give definition and with as much flexibility as necessary to provide for acceptable alternatives. It would no longer be necessary to separate all the precise provisions and call them recommended practices or acceptable means of compliance. All would have equal validity, as recommendations, while any degree of preference could be written into the code. I.C.A.O.'s contribution to future aircraft development would be as great—in fact greater—without the tortured attempt to cloak its findings in the semblance of compulsion. Obligatory international standards can only follow, they cannot lead, technical development.

While the international code of airworthiness, having all the moral force and all the utility of the present so-called standards, would continue to influence future aircraft, and to serve as a model for national regulations and purchase specifications, Part II of the Annex would remain as standards. The principles and methods to which a State must adhere to secure recognition of its certificates abroad would remain obligatory. Freed from the conflicting aims of present safety and future progress, the task of establishing international standards of airworthiness would be more rationally approached. If the protection of Article 41 were removed, all those standards concerning the aircraft, which are considered essential for present safety, would be adopted without the necessity of disguising them as operational standards, administrative standards or rules of the air standards, in order to avoid the bar to " airworthiness " standards. There is no very clear dividing line—at least, there is no common agreement as to where it lies. A simple fire precaution, for example, which might be regarded as an airworthiness matter by some, may be equally important for the public safety as, for example, a sensitive altimeter, which takes its place, unchallenged, among the operations standards. Without Article 41, the test which decides whether an international standard should be adopted and when it should be applied would be whether it is necessary and whether it is practicable, not its arbitrary classification. Such international standards would remove some of the justification of States for imposing their own national requirements on foreign visiting aircraft, to enforce conformity with their own concepts of safety.

A New Aim

Further effort by the technicians on the formulation of the standards themselves can hardly achieve success. The remedy lies in the Convention, and demands a more radical approach by the contracting States than they have, up to now, been ready to take. The Council, by majority, has neglected to carry out the mandate of the Assembly to interpret and, if necessary, to initiate action to amend Article 33. The Council, also by majority, has declined to examine the meaning and consequences of the penal clauses of Chapter VI of the Convention and to consider what amending or other action is necessary. The contracting States, over-riding a strong minority, refuse to consider any amendment of the Convention, and are prepared to suffer its defects rather than face a domestic legislative problem. A different attitude could have saved much labour in the past. A different attitude is necessary now, if results are to be achieved in the future.

Article 33 should be amended, so far as necessary, to make clear its purpose and its intention that, until applicable standards have been adopted under the Convention, States will recognise certificates of airworthiness, based on national standards, issued by other contracting States. When the only airworthiness standards that are adopted under the Convention are really intended to be applied, and do apply, to the aircraft that are flying internationally, this Article, making international flight dependent on compliance with international standards, will have more meaning.

The penal clauses—Articles 39 and 40—should be removed. They have not become operative and, since new aircraft will always be ahead of international standards, they are unlikely to become operative. States have been content to let aircraft fly over their territories, resting on the strength of moral factors as an assurance of the safety of the aircraft. Since no practical alternative has been presented, they will continue to do so.

Article 41 could then be cancelled or modified, to permit the application of such standards as are really necessary for safety, without undue delay. The safeguard for current aircraft embodied in Article 41 is necessary only because of the prohibitory powers conferred by Articles 39 and 40. If airworthiness standards had the same force as all others, they would carry the same protection against their impracticable imposition.

It might be objected to all this that such unreserved recognition of the fact, that States rely on the good faith of others in respect of their certificates of airworthiness, would result in laxity and a reduction of safety. If make-believe is a factor of safety, perhaps so. In fact, however, there would be more real safety factors. If they were not enough, it would be possible to borrow something from the Havana Convention. The Convention could reserve the ultimate right of a State to withhold recognition from a certificate of airworthiness, coupled with a requirement to show good cause for having done so.

When the Convention is so remodelled as to make it practicable for any international standards of airworthiness that are adopted to contribute to the Convention objective of safety in international flight, the way will be clear to pursue the present objective, that is the perfection of future aircraft. An advisory international code with this object would be more complete, because more readily agreed, than one that attempts to compel uniformity, where uniformity is not essential to safety. It would serve equally the other objects for which there is and can be no compulsion imposed by the Convention —a basis for purchase specifications; a basis for national regulations; and, in consequence, a basis for the certification of imported aircraft.

The technicians have taken a new aim—but the target is ill defined. Only by a review of policy by the States can both the target and the aim be controlled. The Convention is the medium, and to harness its full potential for civil aviation demands something more of the flexible attitude of I.C.A.N. between the wars than that prevailing today.

Aeroelastic Problems in connection with High-Speed Flight

by

E. G. BROADBENT, M.A., A.F.R.Ae.S.
(Royal Aircraft Establishment, Farnborough)

SUMMARY: A review is given of developments in the field of aeroelasticity during the past ten years. The effect of steadily increasing Mach number has been two-fold: on the one hand the aerodynamic derivatives have changed, and in some cases brought new problems, and on the other hand the design for higher Mach numbers has led to thinner aerofoils and more slender fuselages for which the required stiffness is more difficult to provide. Both these aspects are discussed, and various methods of attack on the problems are considered. The relative merits of stiffness, damping and massbalance for the prevention of control surface flutter are discussed. A brief mention is made of the recent problems of damage from jet efflux and of the possible aeroelastic effects of kinetic heating.

1. Introduction

Nearly ten years ago Professor A. R. Collar gave a lecture[1] to the Royal Aeronautical Society entitled "Aeroelastic Problems at High Speed," and the present paper is a review of the developments in the intervening period. If we think back to the state of aeroelastic work ten years ago and compare it with the present state, the main impression is of the vast increase in technical effort on aeroelastic work in the aircraft industry, and the associated development in computational aids. These changes have meant that the flutter calculations carried out in Great Britain in a typical week of January 1956 would have taken about ten years to complete by the staff and equipment available in January 1946. The flutter calculations of today are in fact larger than they were ten years ago, and whereas then they were carried out in exceptional circumstances only, they are now carried out by all firms as a matter of routine.

Unfortunately it cannot be said, as a result of this increased effort, that flutter incidents have been comparably fewer in the past two or three years than they were in the same period before 1946. This is not because new flutter problems have been introduced, for aileron buzz and other compressibility effects were known ten years ago[2], but mainly because the flutter problems then in existence have become more difficult to solve with the thinner aerofoil sections of supersonic aircraft, and because flutter coefficients, particularly the aerodynamic ones, cannot yet be calculated with confidence. Nevertheless, valuable advances have been made in experimental work, both on models and in full scale, so that the aircraft designer has at his disposal the possibilities of extensive flutter calculations, low speed wind tunnel tests of a flutter model, rocket model tests, and flight flutter tests, none of which facilities were used, except very rarely, ten years ago.

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The increase in effort on other aeroelastic work has not been so spectacular, but has continued steadily. Aileron reversal is still the commonest criterion for wing stiffness, but tailplane stiffness is no longer always governed by considerations of longitudinal stability and control, particularly when there is no separate elevator. Divergence is perhaps rather more important for aircraft of the future than it appeared to be in 1946, but it has not yet proved to be a serious menace. Buffeting and engine noise effects may be expected to become more important as may the aeroelastic effects of kinetic heating, but these subjects can only be touched on in the present paper.

NOTATION

A	aspect ratio
a_1, a_2	aerodynamic derivatives
B	dimensional parameter depending on ρV^2
b	wing span
C_L	coefficient of lift
C_m	coefficient of pitching moment
c_m	mean chord
c_β	aileron chord (ft.)
d	depth of section
e	distance of flexural axis behind aerodynamic axis (chords)
F	net damping force
g	fraction of chord of inertia axis aft of leading edge
H_s	structural damping
h	fraction of chord of flexural axis aft of leading edge
h_a	maximum negative value of h_j
h_j	direct aerodynamic damping derivative
k	wing taper
k_1	constant (depending on plan form, type of construction, material used, etc.)
k_2	further constant
L_F	flexibility
L_R	lift from rigid surface
l_ϕ	flexural stiffness
M	Mach number
m_ϕ	torsional stiffness

p velocity of roll due to aileron angle
q_r generalised co-ordinate corresponding to α_r
r non-dimensional stiffness ratio measured with structural axes $= \dfrac{l\,c_m^2}{0{\cdot}81 m_0 s^2}$
s aerodynamic semi-span, root to tip (ft.)
s_a aileron span
t skin thickness
U strain energy in a linear torsion mode, torsion about the centre line of the box with an amplitude of one radian at the tip (lb. ft.)
V forward speed
V_D divergence speed
V_d design diving speed ($M \cos \Lambda \leqslant 1$) (knots E.A.S.), (*see* flutter criterion, Section 8)
V_s speed of sound
α angle of incidence
α_r incidence distribution in mode r
β sweepback of centre line of the box
Λ sweepback of leading edge
ξ aileron angle
ρ air density
σ_w wing density
$\tau = \left(1 - \dfrac{\text{Tip chord}}{\text{Root chord}}\right)$
Ω aileron frequency (cycles/sec.)
ω_a tailplane rotational frequency

2. Aileron Reversal

Ten years ago it might have been supposed that there would be no aileron reversal problem left on high speed aircraft by 1956: that designers would have found some other way of providing lateral control. The avoidance of aileron reversal is what decides the wing torsional stiffness of an aircraft more often than not, and it certainly seems that the increase in wing weight which this demands is an expensive price to pay for the retention of a trailing edge type of control. Conventional ailerons are, however, still almost universally adopted, so that the problem remains undiminished in importance.

2.1. THE AERODYNAMIC DERIVATIVES

Unquestionably the most important step in calculating the aeroelastic loss in rolling power is that of estimating the aerodynamic derivatives, particularly at transonic speeds. A great many experimental results have been obtained by the Free Flight Group of the Aerodynamics Department at the R.A.E. These results have often shown not only the aerodynamic derivatives themselves, but also the effects of aeroelastic distortion in quite a direct way, since the models could not be made rigid enough to avoid it. A typical result is shown in Fig. 1, taken from the work of Lawrence and Turner. Model 2 was made of wood and had a little flexibility at the wing root attachment; model 6 was made of steel. In other respects the models were identical; the wings were rectangular with a semi-span (root to tip) a little greater than the chord, and with a full span flap of 20 per cent. chord. The adverse effect of wing distortion is clearly seen from this diagram, and is particularly evident at supersonic speeds where the centre of pressure under aileron rotation has moved back well on to the aileron itself. A further example, also due to Lawrence and Turner, is shown in Fig. 2, this time for a swept-back wing. The loss in a_2 at Mach numbers a little over 0·9 is very pronounced in this example and leads to aerodynamic reversal, by which is meant the vanishing of the derivative a_2. Results of the type shown in Figs. 1 and 2, while possessing considerable interest for the specialist in aeroelasticity, are rather embarrassing to the aerodynamicist. If the experimental value of $pb/(2V\xi)$ is all that can be measured, then the aerodynamic derivatives cannot be extracted from the results because the degree of wing distortion is not known. To overcome this difficulty an extrapolation can be made from results for wings of different stiffness as in Fig. 1, or the experimental results can be corrected for aeroelastic effects by basing the correction on theoretical derivatives, as in Fig. 2. It is interesting to note that in Fig. 2, once a_2 has reversed in sign, the aeroelastic effect is to offset the severity of the reversal.

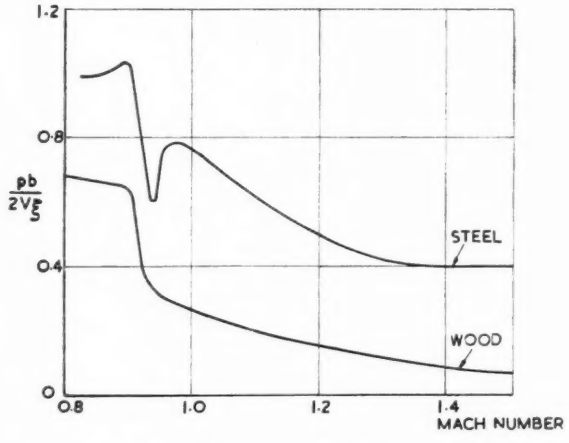

FIGURE 1. Rolling effectiveness of rocket model wings made from different materials.

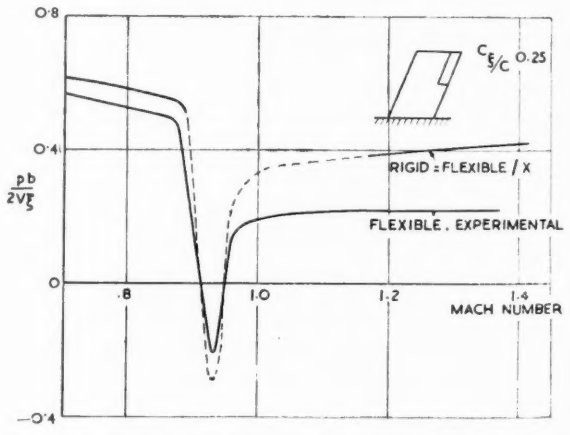

FIGURE 2. Rolling effectiveness of a rocket model wing showing aerodynamic reversal.

2.2. METHODS OF SOLUTION

The calculation of the loss of aileron control due to wing distortion presents no intrinsic difficulty, and engineers would probably get along quite satisfactorily without using a formal solution by the standard method of "try it and see." Thus at a particular flight condition the aerodynamic loads for unit applied aileron would be estimated and hence the resulting structural distortion would be calculated. The next step would be to work out the new aerodynamic loads arising from the distortion and hence the new distortion, and so on until the increments become negligibly small. This process may not be very expedient, however, and a more efficient method must be provided by the use of matrices. The solution for the straight wing of Collar and Broadbent[3] has been extended to cover the swept wing using a matrix iteration process for solution[4]. Radok, however, preferred to work directly with the integral equation[5]. More recently, Williams has given a general solution of the aileron reversal problem in matrix form[6].

These methods, however, although they allow the use of "exact" aerodynamic derivatives, do not help the applicant to determine these derivatives, and it is here that the principal source of error exists. The author has found that it is quite satisfactory to use the old fashioned semi-rigid theory, preferably with three or four degrees of freedom. In fact it is a good plan to use, say, the first four calculated normal modes as generalised co-ordinates, if they are available; this choice avoids the difficulty of what to do about the wing root on a swept-back wing since, in the normal mode calculation, enough degrees of freedom will have been used to represent the root accurately. The theory of Multhopp[7] was used to find the aerodynamic pressure and this in effect gives the incidence distribution in terms of the local lift and moment distribution.

e.g. $$\{\alpha\} = [b]\,\{\gamma\} \quad . \quad . \quad . \quad (1)$$

where the column γ gives the pressures from which the lift and pitching moment are calculated. Equation (1) needs to be inverted to give

$$\{\gamma\} = [b]^{-1}\{\alpha\} \quad . \quad . \quad . \quad (2)$$

Now the work done due to a small displacement δq_r in a mode q_r will be of the form

$$B\,[\alpha_r]\,\delta q_r\,\{\gamma\} \quad . \quad . \quad . \quad (3)$$

where α_r is the incidence distribution in mode r, q_r is the corresponding generalised co-ordinate and B is a dimensional parameter depending on ρV^2. If $\{\gamma_s\}$ represents the lift distribution for mode s, then the work done in co-ordinate s by a small displacement δq_r will be given by

$$\delta q_r q_s Q_{rs} = B\,[\alpha_r]\,\delta q_r\,\{\gamma_s\}\,q_s \quad . \quad (4)$$

hence $$Q_{rs} = B\,[\alpha_r]\,[b]^{-1}\,\{\alpha_s\} \quad . \quad . \quad (5)$$

We choose the first co-ordinate (q_1) to represent the angular velocity of roll, and the last co-ordinate (q_n) to represent unit aileron angle; then by equating the work done in each degree of freedom to zero we obtain $(n-1)$ equations which can be written

$$\begin{bmatrix} Q_{11} & Q_{12} & \cdots & Q_{1n} \\ Q_{21} & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ \cdot & \cdot & & \cdot \\ Q_{n-1,1} & & \cdots & Q_{n-1,n} \end{bmatrix} \begin{bmatrix} q_1 \\ \cdot \\ \cdot \\ \cdot \\ \cdot \\ \cdot \\ q_{n-1} \\ 1 \end{bmatrix} -$$

$$- \begin{bmatrix} 0 & 0 & \cdots & \cdots \\ 0 & \omega_2^2 A_{22} & 0 & \cdots \\ \cdots & 0 & \cdots & \cdots \\ \cdots & \cdots & \cdots & \cdots \\ \cdots & \cdots & \cdots & \cdots \\ \cdots & \cdots & \cdots & \cdots \\ \cdots & \cdots & \cdots & \cdots \\ \cdots & \cdots & \omega_{n-1}^2 A_{n-1,n-1} & \end{bmatrix} \begin{bmatrix} q_1 \\ \cdot \\ \cdot \\ \cdot \\ \cdot \\ \cdot \\ q_{n-1} \end{bmatrix} = 0 \quad . \quad (6)$$

Where A_{rr} is the inertia coefficient in the rth mode (the $(r-1)$th normal mode) and ω_r is the corresponding frequency. If a flight condition (height and speed) is now chosen to fix B, equation (6) can be solved for $q_1 \ldots q_{n-1}$, and in particular for q_1. A graph of q_1 and hence rolling velocity, against forward speed, can then be obtained by solving for several values of B.

2.3. THE AVOIDANCE OF AILERON REVERSAL

The accurate prediction of an aileron reversal speed is only one side of the picture; the other is to answer the question of what design features lead to a high aileron reversal speed without a severe weight penalty. It is the nose-down pitching moment (for aileron applied downwards) that causes the adverse wing twist, and if this pitching moment could be avoided the aileron reversal problem would largely be solved. It should be noted that this pitching moment increases steadily with Mach number right into the supersonic range when the centre of pressure under aileron rotation settles in its mid-aileron position, with perhaps a slight local worsening near sonic speed. Thus aileron reversal is not one of those problems that are much easier to solve on a supersonic aircraft of the type restricted to relatively low indicated speed subsonically.

If the controls are to be manually operated by the pilot, there is little hope of avoiding the adverse pitching moment. On the contrary a suggestion by Williams[8] (not, of course, restricted to a manual circuit) was to make use of this pitching moment by deliberately having an aileron with a low reversal speed. A second aileron would be provided to cope with the low speed range. Clearly there are many practical difficulties associated with a control arrangement of this sort, and there is also the danger that the wing flutter speed may be too low. Whatever the reason there has been no attempt, so far as the author is aware, to make use of this idea in practice.

Since irreversible powered flying controls are almost universally adopted on supersonic aircraft, the sign and magnitude of the hinge moment is of little consequence, and the designer has accordingly greater scope for showing his ingenuity. One possibility which avoids the adverse hinge moment of the trailing edge flap is to use

a leading edge flap. In this case any wing distortion assists the rolling moment of the aileron and reversal is out of the question. There are, however, serious drawbacks, among which may be mentioned the danger of tip stalling and severe buffeting on the wing with the up-going aileron, and also the danger of wing-aileron divergence. The latter becomes a serious danger when the aerodynamic centre is well forward as it is at high subsonic speeds.

If a leading edge control is ruled out, the next possibility to be considered is that of an all-moving tip, which, if it does not provide a favourable pitching moment, at least avoids an unfavourable one. Provided there is no aerodynamic reversal, *i.e.* complete loss of a_2 over the moving tip, positive rolling power should be maintained at all speeds. Again there are practical difficulties: the tip is less efficient in terms of rolling moment per unit area of surface per degree at low speeds than is a conventional aileron, so that relatively large all-moving tips are required for landing purposes. This, together with the fact that there is a large change in hinge moment between subsonic and supersonic speeds, means that a robust mechanism and strong powered control are needed, and at the same time it is difficult to distribute the load from the tip well into the main wing structure, particularly on a thin wing. It is clear that the all-moving tip leads to its own penalties in weight and drag which must be balanced against those of the conventional aileron; but at least in this case the difficulties do not seem insurmountable.

A compromise between an all-moving tip and a conventional aileron is possible by fitting a trailing-edge aileron with a large horn. Superficially this appears to provide an attractive solution and in some cases it may be found so, but in the author's experience horns are common sources of trouble and have provided one of the leading causes of structural failure in the past twenty years or so. The reasons for this may be sought in the following facts: the horn receives a peak aerodynamic load; it is anchored at its rear end, so that any structural distortion is accompanied by a strongly destabilising aerodynamic force; it provides a suitable vehicle for the stowage of mass-balance which then lowers its frequency into the buffeting zone; its hinge moment characteristics are difficult to predict in advance and may change abruptly at transonic speeds. The type of horn intended as a part all-moving tip, however, may avoid some of the difficulties associated with elevator and rudder horns in the past; certainly on a supersonic aircraft the mass-balance can be deleted if fully irreversible powered flying controls are used.

On certain types of aircraft, particularly those with swept-back wings, the designer can go to the opposite extreme of fitting all-moving tip controls, and use an inboard aileron to reduce the adverse wing distortion. For this scheme to be satisfactory the low speed control has to be adequate, but on a tapered swept-back wing the local lift obtained per unit aileron angle for an inboard position greatly exceeds that for an outboard aileron, thus offsetting the disadvantage of the shorter rolling arm. One recent British fighter, the Folland Gnat, is fitted with an inboard aileron with the main object of avoiding aileron reversal.

Other devices should be given brief mention. One is the use of spoilers instead of ailerons, but again there are aerodynamic drawbacks, and the transonic effectiveness is open to question. Another idea is to use a tab geared as a balance tab so as to combat the adverse pitching moment from the aileron, which it can do as it has a greater effect on pitching moment than on lift. The author, acting on a suggestion of Professor Collar, carried out some calculations on this possibility many years ago, but it proved impossible to destroy the pitching moment without making the aileron very over-balanced aerodynamically, and very inefficient at the same time. Moreover, even if the overbalance could be faced with a power-operated aileron control, the loss in effectiveness of the tab at transonic speeds would probably reduce the gain in wing stiffness to a negligible amount. Finally on a conventional aircraft with all-moving tail (preferably a large one) lateral control could be obtained by differential use of the tailplanes. It is doubtful, however, whether sufficient tailplane travel could be provided to give satisfactory control at landing speeds.

3. Divergence

It is well known that classical wing torsional divergence is unlikely to be troublesome at supersonic speeds because of the aft position of the aerodynamic centre. At transonic speeds the position is very different because the aerodynamic centre can often move forward of the quarter chord while the value of a_1 is still increasing. An aircraft with unswept wings designed to operate near the ground at transonic speeds may therefore find that divergence provides the criterion for wing torsional stiffness. One difficulty is that thin biconvex sections of the type that might be expected in the future with an almost complete torsion box have a flexural axis near to the half chord, so that the negative aerodynamic stiffness is much greater than on a wing with a maximum thickness well forward and consequently a flexural axis at about 35 per cent. chord. The danger of divergence on thin supersonic sections is indicated by the fact that in the design of rocket models for flutter investigations, the flexural axis is kept forward at the quarter chord to avoid divergence; a case of divergence has been recorded with the flexural axis as far forward as this, but the rocket tests in question were of 10 per cent. thick wings on which the aerodynamic centre might be expected to move farther forward transonically than on thin supersonic sections. Another factor which adds to the importance of divergence is that a substantial margin is necessary at the worst flight conditions (*i.e.* transonic speeds near the ground) because of the importance of the secondary effects such as are mentioned in the next section. Sweepback is, of course, stabilising.

Although the danger of classical divergence disappears at supersonic speeds, it is replaced by the possibility of divergence involving chordwise distortion of the section. In this type of divergence the wing

leading edge curls up under the destabilising aerodynamic pressures. The danger is not serious, except on a very thin section, and can be avoided by sweepback of the leading edge, particularly near the wing tip; by cutting off the corner, in fact.

Another divergence problem that some designers give themselves is that associated with control surface horns. The value of horns in combating aileron reversal was mentioned in the last section, so that they cannot be condemned out of hand by the aeroelastician. They are, however, a potential source of divergence, and at speeds below the divergence speed the horn flexibility and induced control surface twist can greatly affect the hinge-moments on the control.

4. Longitudinal Control and Stability

On an aircraft with tailplane and elevator the loss of longitudinal control through tailplane twist can be as serious as the loss of lateral control through wing twist. This provides one good reason for fitting all-moving "slab" tails, *i.e.* without elevator, for then the tailplane is not subjected to an adverse twisting moment and no loss occurs. On the contrary, if the tailplane flexural axis is near the half chord the twisting action will tend to augment the lift on the tailplane (positive or negative) caused by the pilot's movement of the stick.

In longitudinal stability the most important aeroelastic effects are probably those that affect the manoeuvre margin. The high aspect ratio swept-back wing has been one of the worst offenders in this respect, at any rate in theory. The bending flexibility of the wing leads to a loss of incidence near the wing tips under positive g thus causing a nose-up pitching moment (because of the sweepback) which is, of course, destabilising. It was chiefly to combat this effect that Hill suggested the solution of the aero-isoclinic wing[9], in which the flexural axis was designed to be so far aft in the wing section that the incidence change under loading at the quarter chord was reduced to a negligible amount.

If the aircraft is without sweepback, it is the wing torsion that provides the destabilising effect. The loss in stability from wing twist can, however, be balanced by a gain in stability through tailplane twist; in fact if a slab tail is fitted the aeroelastic problem is very simple to a first order of accuracy. Due to a small change of incidence, δz, the increments of lift at the wing and tail are

$$\delta L_R + \delta L_F = \delta L_R \left\{ \frac{1}{1-(V/V_D)^2} \right\} \quad . \qquad (7)$$

where δL_R is the increment of lift from the rigid surface, δL_F is the increment due to flexibility and V_D is the appropriate divergence speed.

It follows that the tailplane lift will be magnified more than the wing lift if the tailplane divergence speed is lower than that of the wing. This means that the net effect of the flexibility is to make $(\partial C_m/\partial C_L)$ more negative which gives an increase in stability; conversely if the wing divergence speed is the lower there will be a net loss of stability. As fuselage flexibility is always destabilising it clearly pays the designer to keep the tailplane divergence speed as low as possible without running into other dangers, if he wishes to offset some of this loss of stability at high indicated speeds.

5. Flutter

The impact of supersonic speeds on flutter has been twofold. First, the change in aerodynamic derivatives from subsonic to supersonic speeds has brought its own new problems, and secondly the increase in slenderness which designers are forced to adopt to maintain performance has increased the difficulty of avoiding flutter even at subsonic speeds. To illustrate the second point first, consider the effect of a reduction in wing thickness-chord ratio from 10 per cent. to 4 per cent.; the former is typical of a high speed subsonic aircraft, and the latter of a fully supersonic aircraft. To compare the direct effect of this change on the natural frequencies, we may suppose the percentage structure weight to be the same in each case and also that it is concentrated to the same extent in the skin. Then for the same type of construction

typical stiffness $= k_1 d^2 t$,

and corresponding inertia $= k_2 t$.

It follows that the typical natural frequency is proportional to the thickness-chord ratio, irrespective of what stiffness or strength is to be achieved by either wing. In practice this result will be modified by many factors, but there remains a strong tendency for the natural frequencies to fall as design Mach numbers increase. Now the study of flutter is the study of how energy can be extracted from an air stream by an oscillating surface, and in practice this is possible for only a finite range of frequency parameter.[10] On this basis many more modes must be considered capable of participating in the flutter of the supersonic aircraft—even for the same speed range—than of the subsonic aircraft.

Another indirect effect of the design for high speeds arises from improvements in structural design. The general tendency here is that stress distributions are more even and that non-stress-bearing components are far fewer, both of which lead to low structural damping. Now certain types of flutter are readily prevented by a little structural damping in the aircraft modes, and this "hidden margin" (since flutter calculations very rarely took account of structural damping) must have saved many aircraft from flutter at moderate to relatively high frequencies in the past.

The most important direct effect of supersonic speeds is the introduction, at certain speeds and frequency parameters, of negative direct aerodynamic damping. This can lead to flutter in one degree of freedom, particularly of control surfaces and all-moving tailplanes, which will be discussed in later sections. Quantitative information on these phenomena is at present very scarce, but it is nevertheless necessary for the designer to have some idea of how he intends to avoid this type of flutter on a supersonic aircraft.

5.1. WING FLUTTER

There has been no established case of wing flutter in Great Britain for very many years. The only failure in which wing flutter has been seriously considered since Collar's review[1] nearly ten years ago was that of a towed target, but the evidence was very inconclusive. The prediction of wing flutter, however, occupies a good deal of the effort of the flutter specialist because it can seriously influence the wing structure weight. There is good evidence that this effort, which has only become fully established comparatively recently, is leading to the adoption of smaller margins on wing flutter than in the past. Only a few years ago it was a foregone conclusion that a wing flutter calculation, based on measured ground resonance modes, would yield a flutter speed anything from 50 to 100 per cent. above the design diving speed of the aircraft. In the past two years, however, the author can think of at least three cases in which the margin on wing flutter as calculated after ground resonance tests was below the required limit of 25 per cent.

5.1.1. *Mach number effects*

The accumulation of information on flutter at supersonic speeds is steadily growing but is still too sparse to give a definite rule as to the relative importance of subsonic and supersonic speeds. Most of the results available have come from rocket tests in the U.S.A.[11] and Great Britain, and the usual trend is indicated in Fig. 3. In this diagram the ordinate is the measured flutter speed divided by a reference speed obtained from calculations ignoring compressibility and made with the same assumptions throughout. The reason for dividing by this factor is that the models designed to flutter at different Mach numbers cannot be made identical; the effect of the factor is that the graph shows the influence of Mach number alone as far as possible. The abscissa is the true Mach number.

Results of this type are ideal for turning into a criterion. In a paper written ten years ago, Collar, Broadbent and Puttick[12] quoted the then latest flutter speed formula

$$\frac{1}{V_c}\left(\frac{m_\theta}{\rho s c_m^{\,2}}\right)^{\frac{1}{2}} = \left(\frac{0{\cdot}85\,(g-0{\cdot}1)\,(1{\cdot}3-h)}{(1-0{\cdot}8k+0{\cdot}4k^2)\,(1-0{\cdot}1r)}\right)\phi\,(M) \quad (8)$$

where $\phi\,(M) = (1-M^2)^{-\frac{1}{2}},\ 0 < M < 0{\cdot}8$
$= 1{\cdot}29,\ 0{\cdot}8 < M$

The Mach number term was not quoted in the original expression but was added to the criterion suggested later in the paper. Also d has been replaced by s.

Soon after this, Molyneux's wind-tunnel experiments (at low speed)[13] showed that the taper factor could be linearised and the effect of sweepback could be included quite simply. Subsequent attempts to bring the criterion up to date have mostly been directed at the Mach number term[14] which in equation (8) is confined to the subsonic range. It will be seen from Fig. 3 that when the transverse Mach number exceeds unity the trend is for the flutter speed to rise, apparently sharply. The

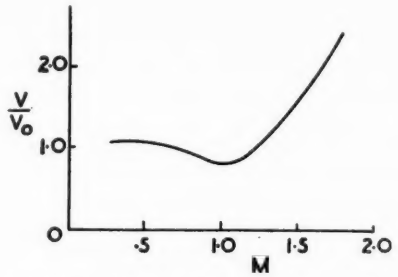

FIGURE 3. Flutter speed—Mach number curve.

evidence for this rise is unfortunately not very precise; the rising part of the curve is in any case difficult to explore experimentally, and in one of his latest suggestions Molyneux proposes a formula which has a linear factor with Mach number; the flutter speed is given by V_2 where

$$V_2 = V_1\,(1 - 0{\cdot}166\,M_1 \cos\Lambda) \quad . \quad . \quad . \quad . \quad (9)$$

$$V_1 = \left(\frac{m_\theta}{\rho s c_m^{\,2}}\right)^{\frac{1}{2}} \left\{ \frac{(0{\cdot}9 - 0{\cdot}33k)\left(0{\cdot}77 + \frac{0{\cdot}1}{r}\right)\left(0{\cdot}95 + \frac{0{\cdot}1}{\sigma_w}\right)}{0{\cdot}78\,(g - 0{\cdot}1)} \right\} \times$$
$$\times \left\{ 0{\cdot}9\left(1 + \frac{0{\cdot}8}{A}\right) \sec\left(\Lambda - \frac{\pi}{16}\right) \right\}$$

The principle is straightforward. First compressibility is neglected and a flutter speed V_1 obtained. This is then converted to a Mach number (M_1) by dividing by the speed of sound, and the true flutter speed V_2 is calculated by a linear relation with M_1. The limit of the criterion is fixed in relation to M_1 which may be considerably greater than the true Mach number at flutter. It will be noted that the formula (9) also contains an aspect ratio correction; this is important for the low aspect ratios which must be considered for supersonic aircraft. A further brief discussion of criteria will be given in a later section.

The principal use of formulae such as (9) is in the project stage of aircraft design, where a simple equation for the flutter speed is essential when exploratory calculations into different layouts are being undertaken. As soon as the project takes definite shape, a more accurate prediction is necessary. The fault of simple semi-empirical formulae (or criteria) derived from rocket tests is that they are based on models with idealised structures and mass distributions, whereas the wing structure and mass distribution in practice will be non-uniform and influenced by the fuselage attachments. Flutter calculations enable account to be taken of these properties of the aircraft, but in the project stage time is short and flutter calculations at high Mach numbers and supersonic speeds take a very long while to carry out. The best solution in practice is to compromise by calculating the flutter speed for incompressible flow and allowing for the effect of Mach number by direct use of experimental results plotted on diagrams of the type of Fig. 3.

5.1.2. *Sweepback*

The reason for using sweepback on high speed aircraft is to delay the adverse effects of compressibility. It should not be forgotten, however, that the favourable effects of compressibility are also delayed. If it is accepted that the worst condition for flutter on a supersonic aircraft is given by a constant value of $M \cos \Lambda$ just less than unity (this is not yet an established fact), then the swept wing experiences its worst conditions at a higher forward speed than the unswept wing. Two subsonic aircraft, one swept and one unswept, may be compared in terms of the required torsional stiffness to prevent flutter

$$\left.\begin{aligned} \bar{m}_\theta &= k_3 V_d^2 \\ \dot{m}_\theta &= k_3 V_d^2 f(\Lambda) \end{aligned}\right\} \quad . \quad . \quad (10)$$

where the bar and the circumflex are used to denote the unswept and swept wings respectively.

For the two supersonic aircraft, in which $V_d \cos \Lambda$ is greater than the speed of sound V_s the equations comparable to (10) are

$$\left.\begin{aligned} \bar{m}_\theta &= k_4 V_s^2 \\ \dot{m}_\theta &= k_4 (V_s \sec \Lambda)^2 f(\Lambda) \end{aligned}\right\} \quad . \quad (11)$$

so that $(\dot{m}_\theta / \bar{m}_\theta)$ is increased by the factor $\sec^2 \Lambda$ in going from the subsonic to the supersonic aircraft. It follows that if the prevention of wing flutter is in fact the design criterion for the wing, then either the swept wing has a very great advantage for subsonic aircraft or else it must carry a serious weight penalty if used on a supersonic aircraft. If (still assuming that $M \cos \Lambda \doteqdot 1$ defines a worst speed for aircraft capable of moderate supersonic speeds) at still higher speeds conditions more favourable to the start of flutter exist, say at $M \cos \Lambda \geqslant 2$, then the situation would be reversed again and the unswept wing would be at a disadvantage. There is, however, even less evidence about speeds as high as this than there is about transonic speeds.

Sweepback on a supersonic aircraft appears to show certain tangible advantages in the prevention of control surface flutter, as discussed later, but in this case it is sweepback of the hinge-line that is significant so that a delta wing would show little benefit.

5.1.3. *Body Freedoms*

When sweepback was introduced on high speed British aircraft a few years ago, a possibility that caused serious concern was that of flutter involving only wing bending and the aircraft body freedoms. It was shown theoretically that this could occur, particularly on aircraft of moderate sweep,[15] and the flutter speed could be relatively quite low. Some comprehensive wind tunnel tests carried out by Gaukroger,[16] however, showed that this type of flutter is suppressed by quite a small tailplane. The effect is shown in Fig. 4 where the boundaries for body freedom flutter are shown on a plot of fuselage moment of inertia against sweepback for different sizes of tailplane. The largest tailplane shown, which has almost eliminated the body freedom region of flutter, is only about half the size of a conventional aircraft tailplane. The conclusion from this work is that body freedom flutter is only likely to be a problem

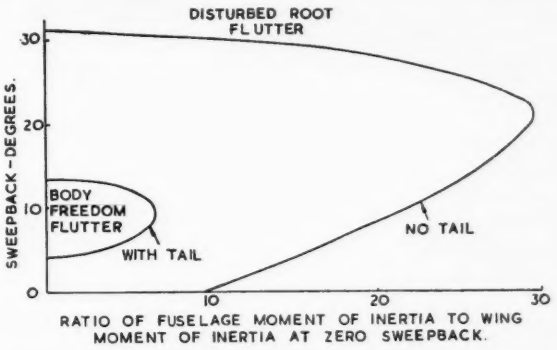

FIGURE 4. Tailplane effect on the boundary between body freedom and disturbed root flutter.

in the design of a tailless aircraft with slight sweepback —the " flying wing " type of layout. There is, however, another possibility: Gaukroger's tests show that for " negative fuselage inertias," a condition represented by restraining springs, body freedom flutter is more likely. The same effect could occur in practice on an aircraft with small wings mounted on a large stiff fuselage—typical of the layout for a supersonic aircraft with the engines mounted in the fuselage.

5.1.4. *External Stores*

The carriage of external stores is not a new problem, but it is only recently that it has given rise to serious difficulties in the prevention of wing flutter. There is nothing peculiar to high speeds in this flutter possibility, except in the very general sense that thin-winged aircraft provide less room for internal stowage of fuel and are therefore more likely to be fitted with tip, or under-wing, fuel tanks. The subject deserves mention since it has been the cause of wing flutter in the United States. In discussing Garrick's paper at the Brighton Conference 1951,[11] Wasserman described such a case of flutter on a jet fighter with tip tanks. The tanks broke away and the wing skin was wrinkled but the pilot landed the aircraft safely.

The small horizontal fins sometimes seen on the back of tip tanks are used to keep the aerodynamic centre on the tank reasonably well aft, as otherwise it is likely to be very far forward relative to the wing, leading to a loss of longitudinal stability and to a reduction of the wing divergence speed. Fins of this sort are sometimes referred to as anti-flutter fins, but the relief they give in terms of wing flutter speed is usually small.

A large number of wind-tunnel tests have been carried out, both in the United States[11] and in Great Britain,[17] on the carriage of localised masses, and the effects, both favourable and unfavourable, are well understood at low speeds. At supersonic speeds there are no test results available, but one potential danger that does not exist to the same extent subsonically may be noted. The presence of a heavy tip mass will greatly lower the wing torsional frequency, and with it the frequency parameter. Now at low supersonic Mach numbers the direct aerodynamic damping becomes negative for low frequency parameters and for certain

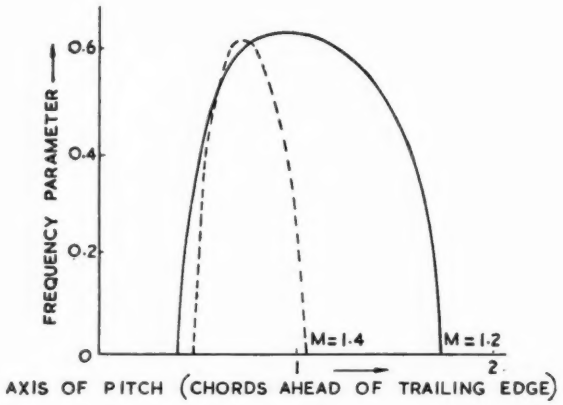

FIGURE 5. Region of negative aerodynamic damping in pitch—two-dimensional flow.

forward positions of the nodal line (*see* Fig. 5). The presence of the tip mass (or a heavy concentrated mass anywhere else on the wing) may therefore increase the likelihood of wing flutter in one degree of freedom.

5.2. TAIL FLUTTER

In discussing tail flutter on a supersonic aircraft, we can safely assume that an all-moving tailplane is involved. Sometimes it will be a simple slab tail, sometimes it will have a geared elevator, and sometimes, even, it may be arranged to follow up on a free elevator. Whatever the arrangement, however, flutter involving tailplane rotation as a principal component must be considered and in fact the design of an operating mechanism with sufficient rigidity presents a major problem. We shall consider first the slab tail, and even here, in spite of the loss of the elevator degree of freedom, the prevention of flutter is not necessarily an easy matter.

5.2.1. *Flexure-Torsion Flutter*

When the primary longitudinal control was by an elevator mounted on a tailplane, the requirements of longitudinal stability and control usually fixed the tailplane torsional stiffness. On a slab tail, however, this does not apply, and it may even be desirable to have a tailplane reasonably flexible in torsion for the reasons given in Section 4. In these circumstances, and in view of the fact that the slab tail does not have to withstand any particularly large steady torsional loads, the torsional material may well have to be fixed by the necessity of avoiding flutter. With a very thin tailplane, such as would be fitted on a supersonic aircraft, considerable skin thickness may be necessary for this purpose even if the cantilever flutter speed was all that need be considered. In practice this is not so and the other degrees of freedom are almost certain to lower the flutter speed.

The main effect of tailplane rotation is to reduce the effective torsional stiffness of the tailplane. We may think of the tailplane having two degrees of freedom, one of tailplane torsion with root fixed and the other of tailplane rotation but rigid in torsion. These two degrees of freedom combine to give two normal modes. In the fundamental, the two types of distortion are in phase with each other and the two flexibilities accordingly are additive; in the overtone the two distortions are in antiphase with a nodal line at about the half span. It is the fundamental mode that is important in the flutter, and a reasonable estimate of the effective stiffness can be obtained by measuring the stiffness of the tailplane on its mounting at about 70 per cent. span. The overtone mode has a very high frequency and is unlikely to be important. If the tailplane is unswept it is found that the position of the axis of rotation is not very important; if anything, the more forward axis positions usually give slightly higher flutter speeds. With a swept-back tailplane both the sweepback and the fore and aft position of the flexural axis are variables, but the best position depends on the effect of the fuselage degree of freedom as well.

5.2.2. *The Effect of Fuselage Flexibility*

Even if the tailplane is rigid in torsion, flutter is possible between fuselage bending and tailplane rotation. In general, therefore, it is necessary to consider four types of distortion in order to get an accurate estimate of the flutter speed, as follows:

(*i*) tailplane bending
(*ii*) tailplane torsion
(*iii*) tailplane rotation
(*iv*) fuselage vertical bending.

If one degree of freedom is allotted to each of these distortions, a calculation in which flutter speed is plotted against tailplane rotational frequency, ω_α (*i.e.* the frequency obtained by varying the stiffness in freedom (*iii*) with the other stiffnesses assumed infinite) is likely to result in a curve of the form shown in Fig. 6. For high values of ω_α the flutter is primarily flexure-torsion in character, the speed falling slightly as the effective torsional stiffness reduces with reduction in ω_α. For relatively low values of ω_α, however, the character of the

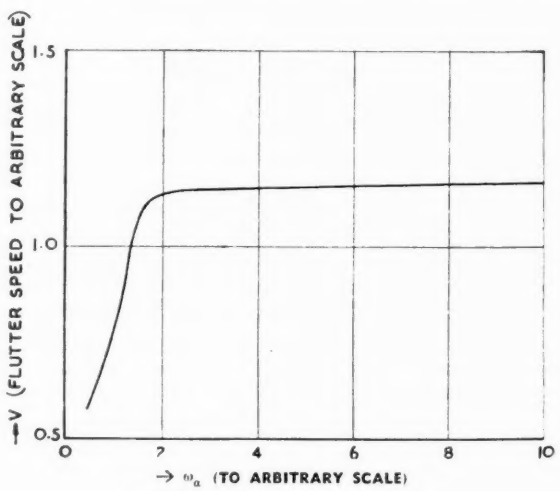

FIGURE 6. Fuselage-tail flutter curve.

flutter changes to something more closely approaching the (*iii*)-(*iv*) binary and at the same time the flutter speed drops sharply.

Clearly the picture given by Fig. 6 depends closely on the frequency ratios of the different degrees of freedom. It also depends on the geometry of the tail unit and on the position of the hinge axis: with a swept-back tailplane the pronounced dip in the flutter speed seems to be avoided if the tailplane is operated about a hinge line normal to the aircraft centre line. This is possibly because the tailplane moment of inertia is so much greater about such an axis that, for a given value of ω_a, the associated rotational stiffness is much greater. The number of variables is, however, so great that no very precise rules can be given at present.

We have seen that the tailplane flutter curve depends on the frequencies of the modes (*i*) to (*iv*). On a supersonic aircraft the tendency is for the fuselage to be long and slender if it does not house the jet engines. This means that the frequency of mode (*iv*) is very low, which is perhaps a good thing in itself as far as frequency coincidences go, but this means that the fuselage overtone bending frequency may be in the flutter range. If this is so it may lower the flutter speed and should certainly be allotted a degree of freedom in the flutter calculations. On another aircraft the tailplane may be mounted on the top of a large swept-back fin; with this type of layout the fore-and-aft flexibility of the fin, or more likely of the fuselage-fin attachment, may be important and should be allowed for. The important lesson to be learnt from this discussion is that if the tailplane torsional stiffness *is* fixed by flutter considerations, it is not sufficient to design on the basis of an adequate cantilever flutter speed and hope for the best: some allowance must be made for the fact that the various flexibilities of the fuselage and attachment will lower the flutter speed. Equally the tailplane rotational stiffness needs to be designed with a reasonable margin in hand.

So far we have considered only symmetric flutter. With this type of layout anti-symmetric flutter is equally likely, since the anti-symmetric rotational stiffness is likely to be of the same order as the symmetric. The anti-symmetric fuselage freedom involved is fuselage torsion. This is likely to have a much higher natural frequency than fuselage bending, but not high enough to be above the range of likely flutter frequencies; in fact there is a danger that a near coincidence in frequencies may cause a serious drop in flutter speed. In the anti-symmetric tail flutter calculation there is one advantage over the symmetric, for it is unlikely that more than four degrees of freedom need be included. The fuselage overtone torsion mode will have too high a frequency to be important, and at the other end of the scale the body freedom (roll) will be heavily damped by the aerodynamic damping of the wing. If the tailplane is mounted on top of the fin it will usually be unnecessary to include a separate mode for fin bending, the fundamental coupled mode between fuselage torsion and fin bending being sufficient.

In general, a calculation of tail flutter will need fewest degrees of freedom if the normal modes of the fuselage and tail together are used, because some of the normal modes resulting from combinations of the simple arbitrary modes (or branch modes) will be pushed outside the flutter frequency range. If this is done it is important that no mode in the range should be overlooked. If one is not restricted in the number of degrees of freedom, then it is probably better to use simple arbitrary modes, or branch modes, because of the greater ease of making specific variations. In choosing the degrees of freedom it is not quite so easy to decide what assumptions to make at the front end, at least in a symmetric calculation; in an anti-symmetric calculation it is probably sufficient to regard the wings as rigid and held fixed. One possibility is to use the symmetric normal modes of the whole aircraft with full allowance for the aerodynamic forces on the wing, but in that case it becomes necessary to include all the wing modes in the range as well. If on the other hand the wings are assumed to be held fixed, the fuselage modes will have the wrong shape and frequency; and if branch modes are introduced for the wing, the symmetric body freedoms must be brought in as well (otherwise there will be no coupling between the wing and fuselage) and so the number of degrees of freedom increases. The problem needs careful thought and each case must be treated on its merits.

5.2.3. *Tails with Geared Elevators*

In principle, a geared elevator need not be a separate degree of freedom any more than a geared tab needs to be, but in practice it is not necessarily any easier to make the elevator gearing rigid than it would be to make a separate powered control rigid. If the elevator gearing is flexible enough to permit separate motion of the elevator in the flutter frequency range, the problem becomes very complicated, and cannot be analysed in detail here. A few points regarding the elevator, however, deserve to be made. In the first place it is useless trying to massbalance the elevator against tailplane rotation, or tailplane torsion. To do this would involve getting the elevator c.g. well ahead of the elevator hinge line, and in view of the large mass needed, the difficulty of housing it would be well nigh impossible to overcome. Furthermore, a heavily overbalanced elevator on a flexible restraint represents a destabilising unit for longitudinal stability.. Some massbalance may, however, be beneficial for dealing with fuselage bending and tailplane bending. As regards the elevator frequency, it may be desirable to keep it fairly low, if it cannot be made sufficiently high, so as to avoid a coincidence with the tailplane rotational frequency at all costs. Again, however, each case must be treated on its own merits.

Finally, on a supersonic aircraft, the direct aerodynamic damping on the motion of tailplane rotation can go negative, thus bringing in the possibility of flutter in one degree of freedom. The problem is, however, essentially the same as the problem of negative damping on a control surface which is more often met in practice, so that the two will be discussed together in a later section.

5.3. FIN FLUTTER

Flutter may be of importance even on a plane fin which does not support a tailplane. Often the fin has a lower aspect ratio, and sometimes a greater thickness-chord ratio, than the horizontal surfaces, and in these circumstances there is little difficulty in meeting the flutter requirement. If, on the other hand, the fin has the same thickness-chord ratio and aspect ratio as the wing, then it may well need to be stiffened to prevent flutter. It is when the fin carries a high tailplane, however, that the difficulty of preventing flutter can become really acute. The problem is essentially that of a wing carrying a heavy tip mass, but there are additional complications arising from the flexibility of the fuselage and even from the possibility of a flexible attachment of the tailplane to the fin. Moreover, the cure, if a low flutter speed is predicted, may not be so obvious, as it may depend closely on the frequency ratios. As the moment of inertia of the tip mass increases the torsional frequency falls more rapidly than does the bending frequency and, with the very large moment of inertia of a tailplane relative to a fin, the fin torsional frequency may well be lower than its bending frequency. In these circumstances, stiffening of the fin in torsion only may cause an adverse frequency coincidence and gain nothing in terms of flutter speed. The possibility of flexibility in the fin-tailplane attachment has been mentioned; in general, such flexibility will have an adverse effect on the flutter speed as it will lower the fundamental natural frequencies of the fin, but it is conceivable that flexibility could be introduced in a manner designed to have a favourable effect by separating two near frequencies. The deliberate use of flexibility in this way would obviously need great care.

5.4. CONTROL SURFACE FLUTTER

It is in the field of control surface flutter that Mach number effects manifest themselves most directly. The possible effects themselves will first be briefly described, and then the question of how best to deal with them will be discussed.

5.4.1. *Mach Number Effects*

At high subsonic Mach numbers, shock-induced separation of the boundary layer can occur and when this happens several possibilities of aileron oscillation exist.

(*i*) With the shock well forward, turbulence from the dead-air region can lead to buffeting of the aileron. This will be greatly aggravated at high lift coefficient.

(*ii*) With the shock just ahead of the hinge line there will be a lag in the adjustment of the shock and resulting pressure change due to aileron displacement. This results in a phase shift of the aileron hinge moment such that the effective aerodynamic damping may go negative. Again the effect may be aggravated at high C_L because of the more adverse pressure gradient.

(*iii*) With the shock wave on the aileron nose, oscillations of the shock across the gap can occur simultaneously with oscillation of the aileron.

(*iv*) When the shock wave has moved to the trailing edge and the flow is everywhere supersonic the aerodynamic damping coefficient for aileron rotation can again become negative.

Of these possibilities item (*i*) is a form of buffeting and can usually be distinguished on a flight record by the less regular form of the oscillation. Item (*ii*) is generally described as aileron buzz; it can only occur over a finite range of frequency parameters and sweep-back seems to be strongly beneficial. Item (*iii*) is a particular form of aileron buzz that is sensitive to the design of the aileron shrouds, etc. Item (*iv*) is a potential flow phenomenon at supersonic speeds that can lead to flutter in one degree of freedom, again provided that the frequency parameter does not exceed a certain value.

It is not always easy to distinguish forms (*ii*), (*iii*) and (*iv*) from classical flutter. Certainly the former are sensitive to Mach number whereas coupled flutter is sensitive to equivalent air speed in the normal way, but two examples will show how the difficulty can arise.

Example 1

The flutter is between wing torsion and aileron rotation in which none of the aerodynamic derivatives is significantly affected by Mach number. At sea level, however, the aerodynamic dampings are too strong for flutter to occur and flutter only becomes possible at heights above 30,000 ft. The flutter speed (E.A.S.) is relatively low, being two-thirds of the design diving speed*, but as the flutter can only occur above 30,000 ft. the Mach number limitations on the aircraft prevent the flutter from occurring except over a small range of Mach numbers near the limit of the aircraft, while at higher equivalent air speeds but lower Mach numbers there is no flutter.

At first sight this phenomenon would be put down as Mach number sensitive, and classical flutter would be ruled out of serious consideration.

Example 2

Again the flutter is between wing torsion and aileron rotation. In this case Mach number affects the derivatives, but not significantly the damping derivatives. Between subsonic and supersonic speeds the centre of pressure under aileron rotation moves aft from about 50 per cent. of the wing chord to about 90 per cent. of the wing chord, aft of the leading edge. The nodal line of the wing torsion mode is at about the half chord position, so it is not until the aileron centre of pressure has moved to about 70 per cent. of the chord, or beyond, that the aerodynamic coupling is strong enough to sustain flutter. (*See* Fig. 7).

This phenomenon also is sensitive to Mach number and to some extent the Mach number and equivalent air speed may be interchangeable in that when the centre of pressure under aileron rotation has moved right back the flutter speed (E.A.S.) may have fallen a little.

*Because of a near frequency coincidence, for example.

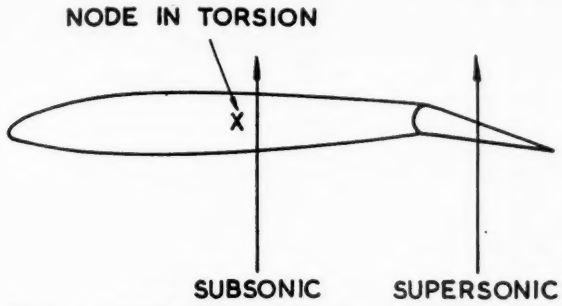

FIGURE 7. Aerodynamic coupling in torsion—aileron flutter.

It may be important to distinguish flutter of the coupled type given in these examples from the essentially one-degree-of-freedom forms indicated by items (*ii*), (*iii*) and (*iv*) since remedies for one (*e.g.* attempts to eliminate the coupling) may make the others worse.

5.4.2. *Massbalance, Stiffness or Damping?*

To the designer of a supersonic aircraft the different forms of control surface flutter all represent additional hazards about which he knows very little in the early design stage, and yet he already wants to decide, in a general way at least, what form of flutter prevention he should provide. Considering the aileron, as typical of all the control surfaces, we may suppose it to be operated by a fully irreversible powered control. It is tempting to make the powered control do the work of flutter prevention, since then all forms of aileron flutter can be catered for at the same time. The first choice, then, is:

(1) *Stiffness*

The aileron is driven directly by the powered jacks from two separate points. Duplication is probably necessary (depending on the type of jack) and if the jacks are separated along the aileron span the possibly adverse effects of aileron torsion are minimised. If a single point of operation must be used, it is better to choose the middle rather than one end. The jacks themselves must be very rigidly mounted: it is essential to keep the dynamic stiffness high. Note that with hydraulic jacks the static stiffness (depending on the valve overlap) should be kept as low as is convenient (large valve overlap) so as to avoid instability of the power system. If mechanical screw jacks are used there is no need for the actual power unit to be near the aileron; it can be housed in the fuselage and the power transmitted mechanically out to the jacks, preferably by means of a high speed shaft.

Probably the main doubt here is whether, in spite of all precautions, the stiffness can be made high enough. Coupled flutter may be prevented without too much trouble; the falling main surface frequencies are a help from this point of view. Negative damping, however, whether of type (*ii*) or (*iv*) is not so easy to prevent because the required frequency parameter is quite high —a rough working rule is that

$$\Omega c_\beta > 100. \quad . \quad . \quad . \quad (12)$$

where Ω is the aileron natural frequency in cycles per second and c_β is the aileron chord in feet.

In fact if a given design meets flutter of type (*ii*) or (*iv*) it is likely to be a more practical possibility to provide artificial damping than to increase the circuit stiffness, which only increases the frequency in proportion to its square root. Again the thin wing makes it difficult to provide a good base for high stiffness. If artificial damping might be necessary anyway, it might be preferable to rely wholly on damping to prevent all forms of flutter from the beginning, *i.e.*:

(2) *Damping*

The circuit stiffness is still kept high, but for flutter prevention reliance is placed on rotary hydraulic dampers built into the aileron hinges. These dampers work against any aileron movement and oppose the pilot as well as the flutter. If sufficient power is available, however, the system can be made acceptable to the pilot.

There are penalties and doubts associated with this scheme also. The penalties are the slower response of the controls to the pilot, or alternatively the heavier power units to provide the same response. The doubts relate both to the possibility of flutter in one degree of freedom, to overcome which the required damping is difficult to estimate, and to the possibility of coupled flutter. In some conditions the amount of damping required to prevent flutter can be very great indeed, so that even when what seems a satisfactory amount of damping has been built into the control, it remains a difficult matter to prove that the aircraft is free from flutter in all conditions. This objection applies, to some extent, against any form of flutter prevention, but it is stronger when the flutter prevention is by artificial damping. With dampers, also, the reliability must first be established; an additional mechanism is being introduced into the aircraft and if it should fail or prove defective in any way, even after considerable flying, an aircraft might be lost as a result.

The third possibility is:

(3) *Massbalance*

The circuit stiffness is not kept particularly high, but care is taken to avoid a frequency coincidence, particularly between wing torsion and aileron rotation. Flutter in one degree of freedom can only be stopped by keeping the aerodynamic damping positive. At subsonic speeds where shock waves are present this might be done by

(*a*) the use of a favourable aerofoil section; on a supersonic aircraft the low thickness-chord ratio alone might be sufficient to avoid trouble.

(*b*) the use of vortex generators. The purpose of these is to put energy into the boundary layer and reduce the region of separated flow; they can certainly cure buffeting (item (*i*) of Section 5.4.1) and are probably effective against some forms of buzz.

(*c*) sweepback of the hinge line.

At supersonic speeds where the direct damping can become negative in potential flow, the two most likely possibilities for avoiding trouble seem to be

(*d*) the use of low aspect ratio control surfaces, since the end effects are expected to be stabilising.

(*e*) the use of considerable sweepback on the hinge line.

The objections to massbalance are that it does not always achieve its own object (the prevention of coupled flutter by destroying the coupling) and that the avoidance of hazards due to negative damping could involve a long and difficult programme of flight clearance, probably coupled with wind tunnel or rocket tests. There is also, of course, a weight penalty. On the credit side there should be no service trouble such as might be experienced with dampers.

The descriptions just given of three different methods of flutter prevention are intended to be illustrative only. Somewhat different methods of attack are possible using any of the three principles, and there are many ways in which they can be combined, *e.g.* massbalance to prevent coupled flutter and damping to prevent compressibility flutter; stiffness and massbalance, however, do not combine very well. There is no certain knowledge as to which of these three principles is best to follow, but the author's personal view is that the first is the right choice together with a provision for fitting a damper at short notice, should it be found necessary later.

5.4.3. *All-moving Surfaces*

Negative damping at supersonic speeds can occur on all-moving surfaces (tips and tailplanes) as well as on control surfaces, and flutter incidents on this type of control have in fact occurred. Stiffness and damping can be used to prevent flutter in the same way as for a control surface, but since the control must be applied at the root, the possibility of distortion of the surface must be considered and can be very important in the case of a tailplane. Massbalance may be effective against coupled flutter, but the weight penalty is likely to be severe.

5.4.4. *Horn Balances*

A powerful horn balance should usually be treated as a separate degree of freedom, especially if it is used as a housing for a massbalance weight. It is, of course, most important that the frequency of the horn in bending should be well above any of the natural frequencies of modes likely to take part in flutter. Even when rigid, horn balances present a problem because of the inadequate knowledge of the aerodynamic derivatives, and since the horn is usually at an anti-node in the flutter motion the effect of the horn derivatives is magnified.

5.5. EXPERIMENTAL WORK

It is not intended to describe here the techniques used in the many fields which exist for experimental work on flutter, but it is necessary to make some assessment of its importance in the flutter clearance of aircraft.

5.5.1. *Ground Resonance Tests*

The part played by the ground resonance tests of a prototype depends on the general philosophy adopted for flight clearance. Three different ways of using the ground resonance tests will be mentioned briefly.

(*i*) The calculations are based throughout on normal modes of the whole aircraft. In this case all effort must be made to measure normal modes in the ground resonance tests. This probably necessitates more than one vibrator to overcome the effects of structural damping, and it may be argued against the method that the effects of local changes in structure are difficult to obtain from the calculations. The artificial subjugation of the structural damping in itself is probably not important but it would certainly be difficult to allow for.

(*ii*) The calculations are based throughout on branch modes—*i.e.* modes of the wing, fuselage, tailplane and fin by themselves. When the ground resonance tests are made, the aircraft normal modes are worked out from the branch modes and compared with the measured modes; local adjustments in stiffness are then made in the branch modes until good agreement is obtained. In this way the resonance tests are merely used to check the initial data. Again the aim of the tests should be at measuring normal modes, but errors in orthogonality are not so important as for (*i*), and structural damping of the components can be introduced if desired.

(*iii*) The design calculations are based on arbitrary modes, and flutter calculations for main surface flutter rely on arbitrary modes throughout. Normal modes are worked out from the arbitrary modes and compared with the ground resonance modes as in (*ii*), but the ground resonance tests are treated differently. No attempt is made to measure normal modes but the vibration is applied near the relevant control surfaces, and control surface flutter is then covered by binary calculations with direct use of the appropriate measured modes. In adapting the arbitrary modes to give good agreement with the ground resonance tests, component structural damping can be included and the modes calculated for the same exciter position as used in the resonance tests.

All these lines of attack are being pursued in practice, with variations, but since this paper is primarily concerned with effects at high Mach numbers an advantage of the last method deserves mention. The main surface calculations are the only ones that run to many degrees of freedom and with this method the modes of distortion are all expressed in simple algebraic forms, which makes analysis easier; this may be an important consideration when time is short and many integrals have to be evaluated for three-dimensional flutter coefficients at high Mach numbers. In general, the value of the resonance tests has not diminished on high speed aircraft, but the results are sometimes used more indirectly than in the past.

5.5.2. *Impedance Tests*

The value and difficulties of impedance testing have provided one of the main sources of argument among flutter specialists in recent years. Early attempts to measure impedance directly gave results that were meaningless; the errors arising from inaccurate phase measurement and the effect of unwanted flexibility in the control surface were such as to swamp the results. Nevertheless, on transonic and supersonic aircraft it is most important to know the impedance of the control circuits, because direct damping and stiffness are primary means of flutter prevention.

The present tendency is for the impedance to be obtained by some form of synthesis. For example, the impedance of the power system as such (the valve and jack) can be measured directly in a rig; this is much easier than on the aircraft because there need be no unwanted distortion and no severe restrictions on the forces used in the test. To obtain the resultant impedance on the aircraft, the jack mounting stiffness must be measured and an assessment made (*e.g.* from the main ground resonance test) of the possibility of structural feed-back through the circuit. Alternatively the impedance may be measured directly on the aircraft, if good instrumentation is available and if the unwanted flexibilities can be avoided.

One of the worst difficulties of impedance testing is that of covering all the adverse conditions, *e.g.* when the pilot is moving the control, or when the control surface is under heavy static load, or possibly even when the jack is stalled and the valve held open. Again some attempt must be made to estimate the impedance under these conditions. In some cases artificial damping may have been built into the power system as a flutter preventive because the task of providing sufficient stiffness proved too severe; it is obviously important to know whether any of this damping is lost under such adverse conditions and if so how much. In a similar way stiffness may be lost, and this also should be checked.

5.5.3. *Flight Flutter Tests*

Flight flutter tests[18] can use techniques either of continuous excitation, in which a vibrator is fitted to the aircraft, or of impulsive excitation either by stick jerking or by explosive charges. Continuous excitation is best, when it can be fitted, since it gives both the amplitude response and the rate of decay after suddenly cutting the excitation. The impulsive technique can lead to difficulties of interpretation particularly when two types of motion are only slightly separated in frequency but possess different rates of decay: the resulting trace will contain beating and might appear to be much more heavily damped than one of the roots really is. Even if the impulsive technique has its limitations, however, it is much better to augment the theoretical work by measurements of this sort than not to augment it at all.

Flight flutter tests, therefore, can be carried out in varying degrees of thoroughness which demand corresponding degrees of time and effort in prototype testing. Two questions need to be answered

(1) To what extent should flight flutter testing become a standard routine for prototype aircraft?

and

(2) How will flight flutter tests cope with high Mach number phenomena (such as are mentioned in Section 5.4.1) where the changes in damping can be very sudden, and only the control surfaces need be affected?

When flight flutter tests were first proposed by the R.A.E. it was recommended that comprehensive tests should only be made in marginal cases. In the author's view there has been no reason to change this recommendation, and flight flutter tests should not be made on all new prototypes, because they take up too much time. Unfortunately it is always difficult to say when a case is marginal, or rather to say when it is not marginal, because of the many uncertainties in the theoretical work. The author's interpretation of when flight flutter tests should be made is:

(1) After a flutter incident has occurred and modifications are being tried out. Here the reason is that flutter clearance has been optimistic once and may be so again;

or (2) When, using the best available information, the estimated flutter speed gives a smaller margin than is normally accepted;

or (3) When the flutter calculations indicate the existence of a mild flutter zone within the flight range but when certain favourable features have been neglected because of their unknown magnitude.

On all new prototypes, however, flight vibration equipment should be carried so that if a mild flutter is encountered it can be recorded.

There is no real evidence as to how flight flutter testing will behave at transonic and supersonic speeds, although at supersonic speeds, once the transonic region is past, there is no reason to expect trouble. The best guidance for transonic tests would seem to be to start at high altitude and gradually work down taking readings through the transonic range. As a simple example in one degree of freedom suppose that at $M=1$, the direct aerodynamic damping derivative on the aileron ($-h_{\dot{\beta}}$) reaches its maximum negative value $-h_a$, say. The structural damping is H_s, and h_a and H_s are positive. The net damping force is

$$F=(H_s-\rho V_s s_a c^3 h_a)\,\dot{\beta} \quad . \quad . \quad (13)$$

At high altitude F will still be positive, and as altitude is reduced the damping will fall linearly with air density. With this technique there should be good prospects of success for coupled problems also.

5.5.4. *Model Work on Flutter*

The use of research models in flutter work has already been discussed in Section 5.1.1, but in addition to this general work, models of specific aircraft are also built with the object of amplifying the flutter calculations on the prototype. These models can be either free-flight models or wind tunnel models, the scope of the two being rather different. A free-flight (rocket) model can be made to fly through the complete Mach

number range of the aircraft but is otherwise rather limited: the model itself is lost after a single flight; the instrumentation is limited; the conclusions are often uncertain (vibration nearly always occurs, but there may be doubt as to whether it is flutter); finally, the variation of parameters is only possible by building a series of models. In principle, wind tunnel models can also be taken to high Mach numbers, but in practice economic considerations have limited them to low speeds in Great Britain. This type of model serves two purposes: it allows close observation of the flutter mode, and it allows easy variation of many parameters. In this way important omissions in the flutter calculations might be detected, and certain variations which are, perhaps, difficult to make in the calculations can be covered.

In the author's view the low speed wind tunnel model usually gives more useful information than the rocket models do, because any doubts, *e.g.* of adequate representation or of mounting, can be fully explored in the wind tunnel. The wind tunnel tests can also investigate parameters such as backlash and other non-linear effects which could not be well represented in a calculation. The other use of a wind tunnel model is that certain structural joints for which the stiffness is difficult to calculate, can be reproduced to scale in the model, so that again the flutter calculation is improved upon. It should not be thought that the flutter calculations can be dispensed with if a model is to be built, because there will be omissions from and uncertainties in the model just as there are in the calculations, only the two will be different and the defects in either should be shown up by a mutual comparison.

As a general policy, models are only needed for pioneering work, *i.e.* when the aircraft to be built is sufficiently unusual in performance, layout or construction, for there to be serious doubts about the theoretical work. There is also little point in building a model if the aircraft structure is not designed by flutter requirements, so that the margins of safety against flutter are expected to be large. In some circumstances a third type of model might be considered whereby no attempt is made to build a representative flexible model; instead the geometry only is representative, and the model is designed for the measurement of aerodynamic derivatives.

6. Buffeting and Other Vibration Effects

Buffeting of the type that arises from boundary layer separation has already been mentioned. The most serious cases in practice are those where the breakaway arises from a junction or fillet, since aerofoil sections on supersonic aircraft will be designed to avoid severe separation—except at very high lift coefficients. Often vortex generators may provide a cure by putting energy into the dead air and hence cutting down the buffeting at its source. This treatment is not always possible, however. Fighter aircraft may have to carry external stores (guided weapons, rockets or even bombs) and bombers also may have to accept strong temporary sources of buffeting, such as open bomb doors. In these circumstances all that can be done at the source is generally to clean up the stores, or improve the bomb door design, but the excitation will by no means be completely eliminated by this process. The resulting degree of response of the aircraft will depend on the degree of damping present in the various structural modes of which the frequencies lie within the range covered by the buffeting source. Modes of low damping can lead to serious amplification of the buffeting.

A mode can be poorly damped either because the damping coefficient is small, in which case the effect will be largely independent of forward speed (E.A.S.), or because two modes such as wing bending and aileron rotation are combining to give a near flutter condition, in which case the effect will probably be sensitive to equivalent air speed. The second type of condition offers scope for improvement by raising the critical flutter speed, but with a mode that is poorly damped at all speeds there is little hope of effecting much improvement. In spite of this difficulty, however, it is a help to know what amount of damping is likely to prove satisfactory in practice. The author's view is that a value of 0·03 times critical damping (based on an analogy with damping in one degree of freedom) should be aimed at for all modes, but examples are known, even in service aircraft, where the damping is as low as 0·02 and even lower. The measurement of these dampings is not always easy, the most reliable method being to cut the excitation during a flight flutter test with a variable frequency exciter, when tuned in to resonance.

A vibration problem of a rather different type, which has recently become important on jet aircraft, is that arising directly from the jet stream. The jet engine provides a source of vibrational energy with a wide frequency spectrum (of the order 100 c.p.s. to 1,000 c.p.s.) and this energy can cause resonances of local structure, particularly skin panels, to an extent that can lead to fatigue failure after a few hours. Serious effects can occur to any structure within a cone whose apex is at the jet exit and whose semi-apex angle is about 30°, so that wing engines mounted either in nacelles or the wing root may cause damage to the tail and rear fuselage. Experimental evidence on the nature and magnitude of the vibration is very scanty as yet, but theory suggests that severity of the excitation increases extremely rapidly with the shear velocity of the jet. The shear velocity is so important, in fact, that for existing aircraft the only danger period occurs when the engines are being run on the ground; but short though this time may be, it is quite long enough to cause fatigue after a few flights in a bad case. The increase of aircraft speed of itself does not make this problem more important, but indirectly the adverse effects during ground running increase rapidly with the increase in shear velocity of the jets, which tend to accompany the increase in aircraft design speeds. In view of the wide frequency band of the excitation there is no neat way of avoiding trouble. The best chance (neglecting other considerations) is to place the engines at the rear of the aircraft so that the structure will not be subjected to the excitation; the alternative is to use

suitably thick and well-supported skins for any affected structure, which may introduce a weight penalty.

7. Aeroelastic Effects of Kinetic Heating

The effect of kinetic heating on aeroelasticity has not so far received much attention in Great Britain. It is to be expected that the design of an aircraft will be such as to avoid serious loss of the elastic moduli of the material since, as the melting point is approached, the strength falls off much more rapidly than the stiffness. In a recent lecture, however, Dryden and Duberg[19] showed how differential heating could cause serious temporary reductions in stiffness, even though the elastic moduli were unaffected. Appreciable loss can occur for a stressed skin wing, but the loss for a solid wing can be very much greater. Consider a solid section, of double wedge shape for simplicity, exposed to a uniform heat inflow. The thin sections at the leading and trailing edges will heat up much more rapidly than the thick centre. Twisting the wing in this condition with hot leading and trailing edges will help to relieve the longitudinal stresses, which means that the wing stiffness has been reduced. A typical theoretical result given by Dryden and Duberg is that for a 3 per cent. thick solid steel wing at 50,000 ft. accelerated at $\frac{1}{2}g$ from a Mach number of 0·75 to a Mach number of 2·75 the effective value of GJ falls to about 30 per cent. in 3 minutes. A loss of this sort could obviously lead to control reversal or flutter on a wing that otherwise had an ample margin of safety. There is, however, no new problem involved on the aeroelastic side; once the effective loss in stiffness has been calculated the aeroelastic calculations can proceed in a standard way. The possibility of direct coupling between aeroelastic and thermal effects does exist, because, for example, warping of the section could occur through differential temperature effects, which would affect the thickness of boundary layer, which would in turn affect the rate of heat flow. This possibility could only affect slow phenomena such as control reversal, and the effect is probably unimportant.

8. Wing Stiffness Design in the Project Stage of an Aircraft

For this work the designer clearly needs criteria in some form or other. Here are three stiffness criteria which attempt to cover wing flutter, aileron reversal (assuming a trailing edge flap as the control) and divergence in the same form.

Flutter criterion

$$U \geqslant 0{\cdot}0035 s c^2 V_d^2 \left\{ \frac{g \cos^{\frac{1}{2}}\beta}{(0{\cdot}77 + 0{\cdot}1/r) \sec^{3/2}(\Lambda - \pi/16)(1 - 0{\cdot}166\, M \cos\Lambda)} \right\}^2$$

where U is the strain energy in a linear torsion mode (lb. ft.), torsion being about the centre line of the box with an amplitude of one radian at the tip (lb. ft.),

and c is the chord (ft.) at $0{\cdot}7s$ from the root if $0 \leqslant \tau \leqslant 0{\cdot}6$, or at $(0{\cdot}775 - 0{\cdot}125\tau)\, s$ from the root if $0{\cdot}6 < \tau \leqslant 1{\cdot}0$ (ft.).

For low aspect ratios (*e.g.* less than 3) replace $\sec^{3/2}(\Lambda - \pi/16)$ by $0{\cdot}9\,(1 + 0{\cdot}8/A)\sec(\Lambda - \pi/16)$.

Aileron reversal criterion

$$U \geqslant 0{\cdot}00096 \frac{V_d^2 c_{0{\cdot}7}^2 s}{\cos^{\frac{1}{2}}\Lambda_{\frac{1}{4}}} \left(\frac{a_1 m}{a_2}\right)$$

V is in knots, $c_{0{\cdot}7}$ is the chord at $0{\cdot}7s$ from root, $\Lambda_{\frac{1}{4}}$ is sweep of $\frac{1}{4}$ chord line.

Divergence

$$U \geqslant 0{\cdot}00112 \frac{V_d^2 c_{0{\cdot}7}^2 s}{\cos^{\frac{1}{2}}\Lambda_{\frac{1}{4}}} (a_1 e)$$

These criteria, which have been developed by the author and Ll. T. Niblett in collaboration with M. E. Burt[20], have been changed in general form to suit early project work. The strain energy in a linear torsion mode is used instead of the stiffness measured under a concentrated torque and in general this linear mode will be a closer approximation to the true distortion mode under distributed aeroelastic loading. In the reversal and divergence criteria, the speed V is the highest equivalent air speed compatible with the Mach number implicit in the values of a_1, a_2 and m which have been used; clearly the criteria must be satisfied for all Mach numbers, not only the highest. The importance of these criteria is very great at this stage of an aircraft, and may decide in a limiting case whether the designer uses steel or duralumin for the wing material; in general, it is probably true to say that steel should be preferred if the majority of the wing is designed by stiffness.

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DISCUSSION

The following is a brief summary of the points made in the Discussion; the spoken lecture was restricted to aileron reversal and flutter, i.e. Parts 1, 2 and 5 of the written paper, and at the end of the lecture a short film was shown of flutter testing by ground-launched rocket made at the Royal Aircraft Establishment. —Editor.

H. S. LINER (*Graduate*): In spite of increased effort on aeroelastic work he thought there was still room for improvement in co-ordination between the different departments of a design firm. The aeroelastic section, for example, was too often presented with a *fait accompli* and told to cure any aeroelastic faults without affecting any other aspects of design.

With very thin sections and low aspect ratios how important was chordwise distortion going to be in the future?

In connection with the design for high circuit stiffness with powered flying controls, he would like to emphasise the importance of keeping the number of moving parts to a minimum. The power unit should also be placed as near the control surface as possible.

As regards experimental research of the type illustrated by the film, he thought that more attention should have been given to control surface flutter instead of wing flutter.

MR. BROADBENT: He agreed that good co-ordination was important, and thought it would be bound to improve with the development of computational aids which would enable much more detailed calculations to be made in the early design stage of an aircraft.

He had made flutter calculations at supersonic speed in which chordwise distortion was taken into account, first using a simple low aspect ratio theory and later using the method of Stewartson from which the flutter coefficients were evaluated for algebraic modes by D. E. Williams of the R.A.E. The first method gave instability to such an extent that no amount of stiffening would cure it; the second method gave no flutter at all. He hoped that some more realistic results would be available soon.

He agreed that moving parts could be kept to a minimum but pointed out that the power unit could be kept remote from the surface provided that suitable compensation were available. The first British aircraft to fly with powered flying controls and no massbalance was the Saunders-Roe Princess, and on this aircraft the power pack was in the fuselage and the control surfaces driven by a high speed shaft through a screw-jack which operated direct onto the control surface. The screw-jacks were statically of high efficiency, but under vibratory conditions proved to be effectively irreversible.

He agreed that ideally more effort should have been devoted to control surface flutter. To make flutter tests with control surfaces, however, meant the investigation of so many parameters that the programme for ground-launched rocket work would have been prohibitive, apart from the difficulties of representation. It was hoped that a programme of derivative measurements, which offered much better prospects, would be under way shortly.

J. M. HAHN (*Graduate*): He welcomed the emphasis placed by the lecturer on high speed computing machines, but was surprised that analogue computors had not been specifically mentioned. He described the role of the analogue computor. Taking up Mr. Liner's first point he felt that the design office in a firm was not stiffness conscious. He thought one of the main problems of the future would be the integration of work on flutter with work on auto-stabilisation involving the interaction of the automatic-pilot and automatic-stabiliser, the powered control, structural distortion and the aerodynamic forces.

MR. BROADBENT: He apologised for not specifically mentioning flutter simulators, the introduction of which had revolutionised the work of flutter calculations. In particular flutter simulators might well prove to be of special value in tackling the general problem mentioned by Mr. Hahn, which included the effect of the various automatic controls and the power unit characteristics, because of the many non-linear terms introduced. He agreed that this was likely to be an important problem

of the future, although not directly related to high speeds.

PROFESSOR A. R. COLLAR (*Fellow*): While expressing admiration for the achievements of flutter simulators and other high speed computing machines, he felt that the results were often obtained in too great a quantity for proper digestion, and there was a danger of too many answers being obtained and too little thought given to their meaning. Referring to the lecturer's remarks on the successful use of screw-jacks (without control surface massbalance) on the Princess, he asked why they were not used more often. With regard to aileron reversal he offered a suggestion he had first made many years ago, that a conventional trailing edge control might be combined with a leading edge flap; he thought that this might avoid the drawbacks associated with either type of control on its own.

MR. BROADBENT: He agreed that care was needed in the use of high speed computors, and on flutter simulators he always encouraged the users to reduce the problem to the bare essentials, after it had first been set up in full, so that a physical understanding of the results could be maintained. In particular this helped to track down possible errors in the computation of the coefficients which was a constant source of worry now that answers were obtained so quickly. The use of direct-acting hydraulic jacks instead of screw-jacks was often preferred on the grounds of design convenience and weight; the hydraulic jack itself could usually be made stiff enough. With regard to the idea of combining leading and trailing edge flaps he knew of at least one firm that was seriously considering the possibility, but he was not yet sure of the conclusions reached.

H. GIDDINGS (Chairman), (*Associate Fellow*): He had always understood that one of the chief uses of a flutter simulator was that it allowed the variation of aerodynamic coefficients and asked whether this was no longer true.

MR. BROADBENT: He agreed that this was an important use of the flutter simulator that was particularly valuable for investigating changes in aerodynamic coefficients (such as loss of control surface damping) at high Mach numbers.

G. C. C. SMITH: He commented that in flight flutter testing it was usual to plot amplitude against air speed and asked whether it would not be valuable to plot modal frequency against air speed as well; an approach to flutter would be indicated by a coalescence of the frequencies. He also believed that the lecturer had no confidence in the use of lifting surface theories (*e.g.* Multhopp-Garner) at high subsonic and at supersonic Mach numbers. He thought that more work should be done to check these theories, against rocket test results, for example, and asked whether there was any specific objection to these theories.

MR. BROADBENT: The idea of plotting frequency against air speed in flight flutter tests was used, he believed, in France and was evidently practical. He felt that the present British technique was more reliable, however, and pointed out that the frequency method was not satisfactory for main surface flutter. On the second point he agreed strongly with Mr. Smith's plea for more comparisons between Multhopp-Garner flutter calculations and experimental results, and said that steps to remedy this lack of evidence were being taken. He only doubted the value of such theories because they had not yet received the backing of experimental corroboration.

MR. HAHN: Taking up Professor Collar's point about the importance of digesting the results obtained from high speed computing equipment, he said that the answer to the difficulty was to let the machine analyse the results for the user's benefit.

MR. N. F. HARPUR (*Graduate*): In proposing a vote of thanks he commented that although the prevention of flutter might be growing more difficult on high speed aircraft, at least most of the problems were the same in essence as they always had been. This was rather in contrast with previous lectures given to the Branch on other problems at high speed where the problems themselves were novel. He thanked the lecturer for a most interesting evening.

Recent Ideas in Plastics for Aircraft Structures

by

J. E. GORDON

(Tube Investments Research Laboratories, Hinxton Hall, Cambridge)

SUMMARY: Strong plastics consist of two-phase arrangements of pairs of brittle solids which in combination may be very tough. Improvement now depends upon an understanding of brittle solid theory. The strength of brittle solids is generally very low but may occasionally approach the very high theoretical values. This is due to the high inherent strength being generally marred by tiny defects. When these defects are removed or contained high strength may be achieved. The various mechanisms by which this may be done are exemplified in mica, common salt, glass, metal whiskers and so on. The relevance of these substances and mechanisms to the manufacture of plastics is discussed.

1. Introduction

Reinforced plastics are two-phase materials, that is to say, they consist of strong particles for so-called reinforcement, glued together with some kind of adhesive which is generally a resin. This fact is apt to be so familiar that one forgets the reasons for it. There is the obvious reason that a mixture of strong particles dispersed in a resin which is at some stage liquid, or at least viscous, can readily be moulded to complicated shapes. It is this mouldability which enables a comparatively high degree of structural efficiency to be achieved, which sometimes outweighs the poor mechanical properties of some of these materials. That is to say, by taking an efficient material which is difficult to shape, one may end with a structure which is heavier than one made from a less efficient material which is easy to shape. We see this point very often in biology because the normal biological materials, that is bone, tendon and muscle, are not really very efficient, but they are used with such skill and economy that many animals such as birds seem to achieve high structural efficiency.

However, this is not the aspect of the problem about which I want to talk. The particles, which are generally used as a reinforcement, have strengths and stiffnesses, which for their weight are better than those of the common metals. However, they are generally aeolotropic and also very brittle, so that even if they could be obtained in large pieces, which is generally impossible, they would be of no use as engineering materials. The two-phase arrangement, that is to say the dispersion of strong particles in a resinous matrix, partly overcomes these objections. To a certain extent we can arrange the orientation of the fibres to suit ourselves, and thus get over some of the engineering difficulties of aeolotropy. Also, although both the reinforcing particles and the resin matrix are generally brittle, the composite material can be arranged to be capable of a certain amount of internal slip, so that some of these materials are very tough. A familiar example is the glass-fibre polyester resin type of plastic which is an exceedingly tough material made from two brittle constituents.

Now, let us try to look at existing plastics with a critical eye, so as to form some kind of an objective assessment of them. We can give them full marks for ease of shaping and for manufacturing convenience. Their specific strength is moderate; their specific stiffness is too low. Their low density, about half that of aluminium, is for most purposes a good point, but I think that the trend nowadays is to need a wider variety of densities for different applications, and the density of most reinforced plastics cannot be varied much. As aircraft speeds go up there is a demand for better temperature resistance. Some of the reinforced plastics such as the asbestos-phenolic materials are very good temporary heat resisters, because the resin chars and forms a protective film of carbon, not unlike that which exists in a tobacco pipe. This temporary protection may be satisfactory for certain weapon applications, but it would be very useful to be able to increase the permanent temperature limit.

Using existing types of fibres and resins it seems to me that the limits of potential improvement are probably in sight, and although these materials can be made a little better, there can be no really great improvement, except perhaps in consistency, which, of course, would be a great boon to the aircraft engineer. If the strength and stiffness are to be improved we shall have to study and to modify the reinforcements, and if better temperature resistance is required we shall probably have to get away from organic resins and look for some other kind of bonding medium. The existing reinforcing particles are already some of the strongest which can be obtained, and it is also fairly certain that our resins cannot be greatly improved by simple chemical modifications. In other words, the limit of development by accepted *ad hoc* methods has been reached.

This seems to imply that the nature and properties of materials must be studied from a more fundamental point of view. Physicists have been theorising about the strength of materials for several generations, but since there appeared to be little practical connection between their speculations and the properties of actual engineer-

A Section Lecture given before the Society on 2nd February 1956.

ing materials, most engineers have not been very interested, and so there has arisen a cleavage of thought between physicists and engineers. There are signs, however, that these two natural allies may perhaps join forces before long. This is partly because the engineer is up against the limitations of what materials can do, and partly because the physicists' speculations are coming a little nearer to reality.

The property about which most is likely to be discovered in the next few years is strength. Perhaps this is rather a pity because I am not sure that stiffness is not a more important property than strength. In fact, although some designers are continually asking for small increases in strength, if they were presented with a really large increase, would they know what to do with it? Of course, the introduction of a really strong material would involve a complete revision of our ideas about structures.

2. The Theoretical Strength of Materials

It is curious that, now that so much is known about atoms and molecules and the forces which surround them, we should still be so ignorant about the strengths of even the most simple and most common materials. A mere integration of the bond strengths in a material generally seems to give an answer which is absurdly high. Here, it is worth mentioning the Polanyi and Orowan[1] free surface energy equation which is:

$$f_t = \sqrt{\left(\frac{2\alpha E}{a}\right)}$$

where α specific surface energy
a inter-atomic distance
E Young's modulus.

When a material breaks, two new surfaces have to be formed, and although the surface tension of a solid cannot be measured it can be estimated from the surface tension of the same material in a liquid state. Polanyi equated the strain energy between two neighbouring layers of atoms or molecules to the energy of the newly formed surfaces, and thus, very simply, obtained this equation. a, the inter-atomic distance, can generally be estimated with reasonable accuracy and will usually be in the region of 3Å. Hooke's law is assumed to operate up to failure and this, of course, introduces an appreciable error. However, in the present state of our knowledge of the subject, we cannot afford to quibble too much, and within a factor of, say, two this equation ought to give a realistic result. If there is any truth in the equation, then the strength of materials would depend on their free surface energy and modulus. Both these properties of course vary considerably, but for the majority of substances which might be considered as potential structural materials, the theoretical specific strengths do not vary enormously, and there is not very much indication that theoretical strength is dependent upon chemical constitution. In nearly every case the observed practical strengths are far lower than theory would indicate but it is of great interest that there are certain exceptions to this. In other words some substances do sometimes show practical strengths comparable with theory.

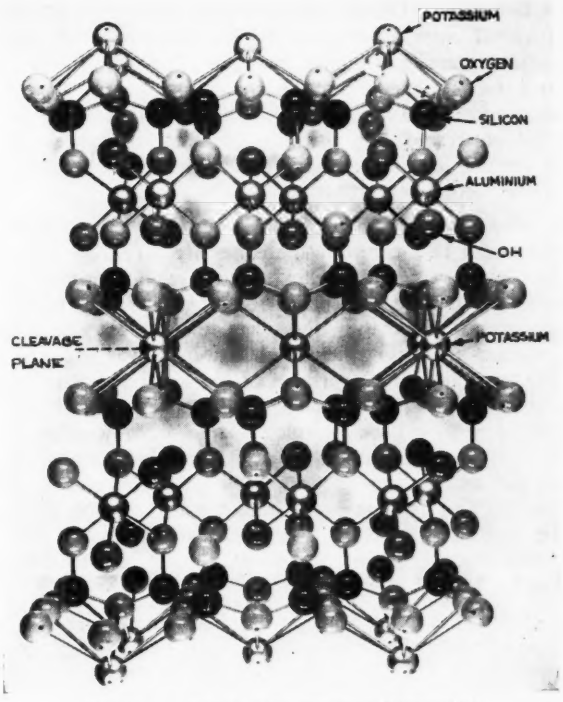

FIGURE 1. Typical mica structure (Muscovite).

3. Strength of Micas and Other Layer Structures

Now, the Orowan-Polanyi equation seems to have a real physical meaning and both its truth and its limitations can be best illustrated by considering the practical example of micas[3] (*see* Fig. 1).

There are a considerable variety of micas, all of which have very similar layer structures. There are comparatively thick continuous well bonded regions separated by planes of greater or less weakness. The simplest case of mica is that of talc where the overall arrangement of atoms in the layers is electrically neutral, and thus there is no charge and therefore no adhesion between the layers. As a result the material has no cohesion and is used in the form of french chalk as a lubricant or for face powder. When aluminium is substituted for one silicon atom in four, the layers are no longer electrically neutral as a whole and a situation occurs where the main layers are separated by potassium ions in such a fashion that the adhesion between the layers is much less than the internal cohesion within the layers; in other words, the material sticks together as a coherent entity, but is readily cleaved in one plane. Such a material is common mica, muscovite.

The mechanical properties of muscovite are very interesting and have been investigated among others

by Orowan[2]. Orowan in his earlier experiments tested strips of mica in tension in the usual manner and obtained tensile strengths between 20-30,000 lb./in.^{2}. In a further experiment he changed the type of test piece and used a much wider specimen so that the edges were unstressed. In this case he obtained figures up to 460,000 lb./in.^{2}, which is perhaps not very far short of the theoretical strength.

The moral of these experiments of Orowan's is clear. No matter how carefully the edges are prepared, when stressed they contain cracks which readily propagate and, as it were, poison the strength of the whole specimen. When the edges were not stressed this did not occur and a very much higher strength was obtained. The two outer (flat) surfaces of the specimen must of course have been fully stressed and it is very improbable that they would be completely free from weakening defects. It seems therefore that defects which originated on the surface were deflected by the weak planes of cleavage and thus prevented from mischief. Thus, when the conditions are such that cracks are effectively prevented from spreading, the material can give something like its theoretical strength.

Now, if proceeding with the substitution of aluminium for silicon in micas, the next stage, margarite, is obtained when two silicon atoms in four are substituted by aluminium and thus the cohesion between the layers (which are separated by calcium ions) is twice as strong as in muscovite. Margarite is weaker and more brittle than muscovite. This is approaching the region of the so-called "Brittle Micas." Fully substituted micas have been synthesised which must be very difficult to cleave and also very weak. In every case the basic molecular structure is virtually identical; the strength depends upon the degree of protection afforded by the cleavage planes. A precisely analogous state of affairs exists with the asbestoses which of course can be cleaved on two planes so as to yield fibres; and, as a matter of fact, both the structure and strength of asbestos is very closely similar to that of mica.

In passing it may be worth reflecting on the usefulness of micas as plate reinforcements in plastics. The strengths and stiffness of a fibrous reinforcement scattered in a plane is something like one-third of that of the individual fibre and, as the packing of such fibres is generally bad, the resin content is usually high, so that in practice one can only get one-fifth or one-sixth of the properties of the individual filament[4]. If a plate is used, not only is the packing probably good, but the full strength and stiffness of the plate might be expected to exert itself in every direction in a plane. *Ceteris paribus*, such a material should be four or five times as strong and stiff as one made from fibres. As the Young's modulus of mica is about 23,000,000 lb./in.^{2} and the strength is also very high, it should form an excellent reinforcement. There are, however, two difficulties which so far seem insuperable. The first is that it seems impossible to avoid stressing the whole width of the reinforcing plates including their edges and, as has been seen, even under the best conditions this reduces the strength by a factor of about 20. The second difficulty is that the cleavage strength of the material in the normal direction would also be low and it would delaminate very easily. All this is very awkward of Nature and it is a great pity that some way of getting around these difficulties cannot be found. It is as if some very strong and stiff playing cards were available but they could only be used in packs, which would come apart readily.

This device of obtaining or rather preserving strength by ready cleavage is a most convenient one in some ways, but in the present state of knowledge it can probably only be used for fibrous filaments of the asbestos type. There are quite a number of substances which form needle crystals which cleave readily.

4. Ionic Crystals and the Joffé Effect

The field of strong non-metallic crystalline materials is a very wide one which has been very little examined. It includes both materials which might be used as reinforcements and also those which might be used as bonding agents. It seems that a wide variety of simple non-metallic crystals are capable of showing high strengths, and that there are other mechanisms besides cleavage by which strength may be attained. Many people are familiar with Joffé's experiments with sodium chloride[5] which have since been extended to most of the alkali halides[6]. If one takes a crystal of ordinary sodium chloride, large enough to test, its tensile strength is very low and is in the region of 700-1,000 lb./in.^{2}. Joffé found that if the crystal is submerged in a solvent so as to wash away an appreciable layer of the surface, the strength is very greatly increased, up to about 250,000 lb./in.^{2}. This effect is apparently not dependent upon the actual wetting of the surface but is connected with the removal of an imperfect surface layer, because the improved strength is retained for some little time after drying. After a period of a few hours or days, however, the strength will return to its original low value. The effect is clearly a surface effect, so that the assumption is that the middle of such crystals are relatively free from defects capable of greatly weakening it, and that the surface is in some way capable of spontaneously acquiring cracks which can poison the whole material. Alkali halides strengthened in this way generally show quite considerable ductility and in fact generally behave rather like metals. They show characteristic slip planes and their strength is much increased by the addition of a very small percentage of foreign ions in a fashion precisely analogous to that of metals.

Silver chloride, for instance, is a highly ductile substance with appreciable strength and it is not at all impossible that ionic crystals of some kind might be developed as high temperature bonding agents.

5. Organic Crystals

High strengths are not confined to inorganic crystals such as salts and minerals and we have recently found at Hinxton Hall that a number of organic crystals also

show high strengths. One is apt to think of organic substances as having low temperature resistance and not very much chemical stability. This is perhaps because most of the organic substances with which one is familiar are derived from living matter and of course the recurring cycles of nature would be impossible if natural structural materials could not quite readily be broken down into their constituents and be rebuilt. In fact when we look at biological structural materials we are sometimes so bewitched by the marvels of Nature that we forget that they are not very good materials from the engineer's point of view. The condition of a natural material is that it should be built up of water soluble constituents and must easily succumb to the digestive juices of whatever preys upon it. Any really durable natural material would be an anomaly which would gradually accumulate in useless heaps about the earth. However, this does not mean that organic materials cannot have high temperature and chemical resistance. This seems to be particularly true of the high molecular weight aromatic compounds which are transitional between classical organic chemistry and graphite. We are all more or less familiar with the organic chemistry of one or two benzene rings, but there is a new, mysterious and most exciting subject in the chemistry of the many-ringed structures. These substances include growth inhibitors, growth controllers and growth perverters which in minute quantities may control the fate of all living matter. The higher molecular weight compounds are generally quite insoluble, have very high temperature resistance and shade imperceptibly into graphite. The specific gravities of such compounds are generally low.

A class of compounds of considerable interest in this connection are the phthalocyanines which in the form of pigments have been known for something like twenty years. These compounds all contain four benzene rings symmetrically placed around a central organic structure with a hole in the middle which can either be left empty or filled by the majority of metal atoms (Fig. 2).

Some of these compounds have a specific gravity in the region of 1·3—1·4. They are thermally stable up to about 580°C. when they sublime without decomposition, and they are extraordinarily inert chemically. They are apparently capable of forming either glasses or large regular asbestos-like crystals which cleave quite readily and seem to have good mechanical strength. However, this is only one out of many classes of compounds which certainly ought to be investigated when looking for new sorts of materials.

6. Metal Whiskers

While on the subject of crystalline reinforcements, it is worth mentioning metal whiskers. It is now fairly widely known that the majority of metals can, under suitable conditions, be persuaded to form long thin fibre-like crystals some of which show very great strength. A pure metal is enabled to slip readily by the presence of dislocations in the crystal so that, instead of every atom having to jump an energy barrier simultaneously, atoms

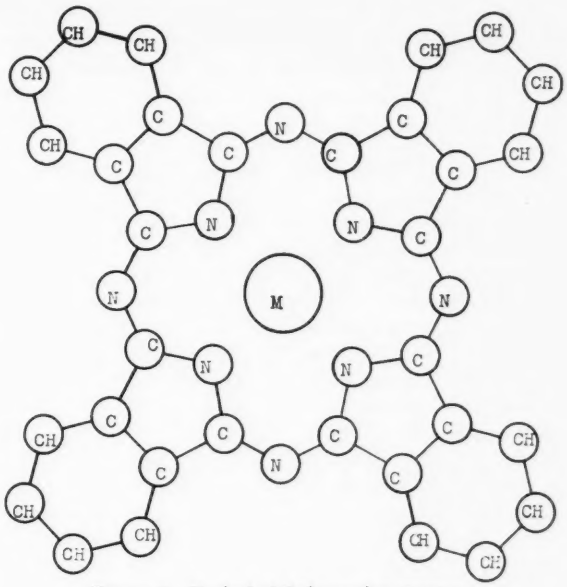

FIGURE 2. Typical phthalocyanine structure.
The central atom can be that of almost any metal.

are able to jump this barrier singly and thus at a much lower mean stress. So pronounced is this effect with most metals that it is necessary to reduce it by introducing foreign atoms which tend to make slip more difficult. It is generally supposed that whiskers consist of virtually perfect crystals which having no dislocations are unable to slip much below the theoretical stress. It may not be quite true to describe these whiskers as having no dislocations because it is supposed by Frank[7] and others that the method of whisker formation is by a growth spiral at the root which winds around a single central dislocation. However, be that as it may, iron whiskers, for instance, having tensile strengths of the order of 1,000 tons/in.^{2} can be made, and while specifically this is not a higher figure than can be obtained with non-metallic materials, if the need is for a dense material, perhaps we might consider seriously the use of iron whiskers bonded together in one way or another, perhaps by a ductile metal matrix.

7. Glasses

It is a curious fact that there is not very much to choose between the strengths of crystalline and non-crystalline non-metallic solids. As a matter of fact higher strengths have been obtained from very fine glassy fibres than from any other substances. I believe the highest figure ever claimed for the strength of any material is about 3,500,000 lb./in.^{2} for fine quartz fibres. In commercial practices we are generally content with strengths of 180-300,000 lb./in.^{2}. I suppose that if the demand were there and the price attractive it might not be too difficult to put on the market considerably stronger fibres. However, the problem facing the manufacturer is generally one of reducing the price rather than increasing the strength. It has for many years been

supposed that the strength of a fibre is connected with its thickness, in a more or less hyperbolic manner and if one considers fibres drawn under comparable conditions this is more or less true. Normally one chooses a diameter which gives a compromise between strength and cheapness. Lately, however, there has been accumulating a good deal of evidence on both sides of the Atlantic that thin fibres are not strong simply by virtue of their being thin. In fact some of us have been able to make thick glass rods which show the strength of fine fibres merely by surface treatments. The evidence is that the middle of even quite thick glass rods is sensibly free of defects and, that, as in the sodium chloride crystals, the practical strength is controlled by the existence of some kind of defect which resides in the surface, and is capable of poisoning the whole. This is presumably the classical Griffiths crack.

So far, only improvements in the strength in glass fibres have been discussed. Unfortunately the Young's modulus of glass is rather low and it is difficult to see how this can be materially improved. However, there are persistent rumours from America that they have been able to obtain considerable increases in E. Another American enterprise is said to be the production of thin glass flakes as a reinforcement. The theoretical benefit of this is obviously considerable and is exactly the same as that which was discussed with mica. Unfortunately, much the same objection seems to hold, which is that one cannot see how the flakes can be used without stressing their edges and the least defect at the edge will weaken the whole flake. However, if there is any way of getting over this, we should immediately be presented with a three or four-fold improvement in the strengths and stiffnesses of glass-reinforced materials.

8. Conclusion

To summarise then, perhaps we are beginning to understand some of the factors which govern the strength of materials and this should lead to considerable improvement in this direction. With regard to temperature resistance and the question of the so-called heat barrier, the materials which have been discussed, which are those with which I am to some extent familiar, may perhaps cater for temperatures up to the order of 600°C. If we want to go higher than this, which no doubt we shall, we shall have to go to the ceramics proper; no doubt the laws which govern their strength and brittleness must be similar to those discussed. Thus a strong ceramic is unlikely to be either isotropic or homogeneous. On the question of density, it is probably important to extend the range which is available to designers. Clearly wings and control surfaces for very fast aircraft and weapons will need to be as thin as possible and will need to make use of very dense materials. The best hope that I can see for this is to develop materials based on metallic whiskers.

At the other end of the scale there is always a need for low density materials and this is one of the reasons why wood remains popular generation after generation. Very few homogeneous materials exist with densities appreciably less than 1·0 and of those which look like being strong the densities are generally in the region of 1·3. If a material less dense than this is required it will be necessary to create voids or cavities of some kind. Dr. Adrian Horridge has examined this problem and comes to the conclusion that there are two alternatives, one of which is to use a foamed resin with a solid reinforcement, and the other to use a hollow reinforcement and a solid resin, or, of course, one could combine the two. He found that, while the prospect for a foamed resin seems very unpromising, the possibilities of a hollow reinforcement were rather interesting and in fact he was able to draw hollow fibres with a wall thickness of about one tenth their radius quite successfully and thus to make materials consisting of fine tubes which were rather similar to wood. No doubt if there were a sufficient demand such processes could be developed into practical propositions.

One of the most important properties of structural materials is their Young's modulus and it is curious that so little is known about the theory of this property. It is perhaps especially curious because it can be reasonably expected to predict the stiffness of a material consisting of many atoms from that of a single pair, whereas we should not expect to be able to predict the strength of such an arrangement from the strength of a single pair of atoms. This is a subject which is waiting to be tackled and on which hardly any work has been done. The mathematical calculations on bond stiffnesses are perhaps not unduly difficult, but the difficulty of the experimental approach is a serious problem.

It would be of immense value to have a kind of catalogue of the stiffnesses of various compounds, that is to say, if we rang the various changes which can be rung on the benzene ring and measured the stiffness of the resulting crystals we could probably predict the moduli of the majority of organic compounds. There is some reason to believe that specifically some of these might be very high indeed, remembering that the specific stiffness of diamond is five or six times that of metals. Unfortunately most of the compounds with which we should like to work can only be obtained in quite small crystals, many of which can only be seen under a microscope and so far no very satisfactory experimental technique has been devised, neither ultra-sonic nor mechanical measurements seem very reliable. However, it is work which will certainly have to be done and some way will have to be found of tackling it.

I should like to end by leaving with you, if I may, two main general impressions. One is that there is an enormous amount which can be done to devise new materials and probably a very wide range of properties and densities could be achieved, if it were thought worth while to do so. Secondly, on the other hand, it is essential to bear in mind that the present research effort on the subject in this or any other country is very small and the size of the problem is very great. It is therefore quite impossible to tackle all the problems and in fact the solution of even a few of them will take surprisingly long. It will take at least as long to develop a new class of materials as a new generation of aircraft.

The present advances in aircraft and weapons could not have been made without a background of a great volume of fundamental research on aerodynamics and structures. Unfortunately the physics of materials has been something of a Cinderella for many years. New materials cannot be obtained merely by demanding them. I am afraid that there is going to be a considerable lag between the present realisation by engineers for the need for better materials and the fulfilment of that demand.

ACKNOWLEDGMENT

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New Zealand Division

Seventh Annual Report: For Year Ended 31st December 1955

STEADY PROGRESS has been made by the Division during the year, both in regard to activities generally and to membership. As remarked last year the major increase in membership is from younger men.

FELLOWSHIP

It has been gratifying to record that during the year three members of the New Zealand Division have been awarded the high honour of Fellowship of the Society, those elected being *Sir Arthur Nevill*, a past President and foundation member of the Division; *Mr. E. A. Gibson*, also a foundation member; and *Mr. G. B. Bolt*, who has been actively connected with aviation activities in New Zealand for over forty years. The award of these three Fellowships can be taken not only as a recognition by the Council of the Society that the activities of the New Zealand Division are in their mind, but also that they recognise that leading members of the aviation industry in this country are taking a valuable part in the world of aviation.

Reference was made in the previous annual report to the need for an organisation to enable the Division to handle trust monies. During 1955 a great deal of work has been accomplished in settling involved legal details and once again the Division must record its gratitude to Mr. G. A. Wylie, our Honorary Solicitor, for his valuable time and assistance on this matter. The draft constitution of the Articles of Association have now been agreed between the various parties concerned and the remaining steps needed are purely formal ones.

It is also with pleasure that we can record that during the year one of the papers read before a Branch of the Division was considered worthy of publication in the Society's JOURNAL. This was the paper "Some Unusual Aspects of Turbulence" given by Captain Bethwaite to the Auckland Branch which contained some original thoughts on this subject. We feel sure that this publication will encourage further contributors who may feel rather diffident because of the relatively small scale of aviation activities in this country in comparison with technical matters of world-wide interest characteristic of the Society's publications.

During the year the Divisional Council directed the attention of the New Zealand Government to the modest scale of activity on aeronautical research in the Dominion.

SIR HENRY WIGRAM MEMORIAL AWARD

For some years the New Zealand Division has offered an award for the most meritorious paper of an original nature given before one of the Division's meetings. This award, which was instituted as a memorial to the late Sir Henry Wigram, has, up to the present, not had any entries, but we are pleased to report that this year papers were submitted by E. A. Gibson and A. Jardine entitled "Aircraft in Agriculture" and "Airline Maintenance Planning and Aircraft Utilisation," respectively.

The recommendation of the panel of judges, that the award should be made to Mr. E. A. Gibson, was approved by Council. The judges also commended Mr. Jardine for the excellence of his paper.

The Division extends its congratulations to Mr. Gibson —the first winner of the award—and to Mr. Jardine for his valuable contribution.

It is hoped that this award will encourage further worthwhile contributions to the New Zealand Division's technical activities.

The Divisional Council is arranging for the actual medallion to be struck in England to our broad design, and is indebted to the Society for the use of appropriate dies already owned by the Society for this purpose.

It was disappointing to find that during the year the Palmerston North Branch was forced to go into recess owing to insufficient regular support. This was largely due to transfers of many active members to Christchurch, but it is hoped that in the coming year this Branch will again be able to function actively.

The Division has once again been greatly indebted for assistance, encouragement and support throughout the year to a number of large organisations, notably Tasman Empire Airways Ltd., the National Airways Corporation, the Civil Aviation Administration, the Shell and Vacuum Oil Companies and the Royal New Zealand Air Force.

In addition house magazines are being regularly received and distributed to Branches from a number of aviation manufacturers and oil companies and the thanks of the Division are due to all these donors.

The Divisional Council for 1955 was elected by postal ballot and from these the following officers were elected:—

President: Air Commodore G. Carter, A.F.R.Ae.S.
Vice-President: Mr. D. A. Patterson, A.R.Ae.S.
Hon. Secretary: Mr. T. T. N. Coleridge, A.F.R.Ae.S.
Hon. Treasurer and Asst. Secretary: Mr. O. A. Kemp.
Council Members: Mr. C. G. Andrews, A.F.R.Ae.S., Mr. B. Cornthwaite, A.F.R.Ae.S., Mr. E. A. Gibson, F.R.Ae.S., Mr. C. W. Labette, A.F.R.Ae.S., Sir Arthur Nevill, F.R.Ae.S., Group Capt. C. A. Turner, A.F.R.Ae.S., Wing Commander Woodward (Wellington); Mr. A. J. Smaill, A.R.Ae.S. (Christchurch); Mr. M. R. Roper, A.F.R.Ae.S. (Palmerston North).

Mr. G. A. Wylie was appointed to be the Honorary Solicitor to the Division.

BRANCH ACTIVITIES

AUCKLAND

Nine meetings, apart from the Annual General Meeting, were held by the Branch and although the annual cocktail party and the film evening were not held through various circumstances, all meetings were well supported with an average attendance of thirty-seven. Branch membership has decreased from 161 to 154 but this was mainly due to a drastic purge of the membership register and this does not indicate in any way a reduction of enthusiasm.

This Branch has an annual Walsh Brothers' Memorial essay competition, and this year the judges reported that a high standard was reached.

PALMERSTON NORTH

Largely owing to transfers of certain members to other districts, particularly Christchurch, it was found necessary for this Branch to go into recess during the year, but it is hoped to open up again in the coming year.

WELLINGTON

During the year the Branch membership has increased from 169 to 175.

Six meetings were held during the year with an average attendance of thirty-eight. The 1955 Branch prize was awarded to Mr. G. B. Bolt, O.B.E., F.R.Ae.S., for his paper "As it was in the Beginning."

CHRISTCHURCH

Membership of the Branch has stayed at 75 although there have been both resignations and new members.

Eight meetings were held during the year including one largely of a social nature. The enthusiasm shown in the resurgence of this branch is a matter for special congratulation to the keenness of its officers.

MEMBERSHIP OF THE DIVISION

	As at January 1955	*As at January 1956*
Fellows	—	3
Associate Fellows	28	29
Associates	64	69
Graduates	7	8
Students	6	7
Companions	4	4
TOTAL	109	120

This membership list includes a number of members who are at present on temporary transfer to the United Kingdom, but most of these transfers have taken place during the year and the members were actually resident in New Zealand for substantial parts of the year.

MEETINGS

The following papers and talks were presented at meetings during the year, some at more than one Branch:—

"Standing Waves over the Southern Alps" by Mr. H. H. Kingham.

"Bird Flight" by Professor H. F. T. Adams, A.F.R.Ae.S.

"Aircraft in Agriculture" by Mr. E. A. Gibson, F.R.Ae.S., Director of Civil Aviation.

"The History and Development of Gliding in New Zealand" by Mr. Georgeson.

"Guided Missiles" by Mr. D. C. Stevenson, A.F.R.Ae.S.

"Bristol Development" by Mr. R. F. Stephen.

"Recent Activities of Shorts" by Mr. E. C. Body, A.R.Ae.S.

"Metal Fatigue and the Factor X in Aircraft" by Mr. Gordon Kells.

"Operation Elba Isle Search and Recovery of Comet Aircraft" by Lt. Commander J. C. K. Harley.

"The Vickers Viscount" by Sir Leonard Isitt, K.B.E., Mr. A. H. Harvey-Bailey and Mr. A. W. Milson.

"As it was in the Beginning" by Mr. G. Bolt, F.R.Ae.S.

"Aircraft Maintenance Planning and Aircraft Utilisation" by Mr. Alex Jardine, A.F.R.Ae.S.

"The Evolution of the Aircraft Engine" by Mr. G. Lindwalker, M.A., A.F.R.Ae.S.

"Lubricating Oil Development and Future Trends" by Mr. L. H. Mirams, A.M.Inst.Aut. and Ae.E.Inc. (Australia).

"Principals of Functional Planning of Airports" by Mr. M. B. Borgeson.

The Division was privileged to receive a tape recording of the Duke of Edinburgh's Commonwealth Lecture and this was presented at several of the Branches during the year. As was only to be expected from such an outstanding paper these meetings were most successful. The Secretary of the Society has promised that tape recordings of other lectures will also be loaned to the Division from time to time and this assistance is greatly appreciated.

In addition a large number of technical and semi-technical films have been shown at all Branches.

DIVISIONAL COUNCIL 1956

The following are the officers and Council for 1956:—

President: Air Commodore G. Carter (*Fellow*).
Vice-President: D. A. Patterson (*Associate*).
Hon. Secretary: T. T. N. Coleridge (*Associate Fellow*).
Assistant Secretary and *Treasurer*: O. A. Kemp.
Hon. Auditor: B. W. Hoult.
Hon. Solicitor: G. A. Wylie.
Council Members: G. B. Bolt (*Fellow*), Air Cdre. G. Carter (*Fellow*), T. T. N. Coleridge (*Associate Fellow*), B. Cornthwaite (*Associate Fellow*), W. H. Dunn (*Associate Fellow*), C. W. Labette (*Associate Fellow*), Sir Arthur Nevill (*Fellow*), D. A. Patterson (*Associate*), M. R. Roper (*Associate Fellow*), A. J. Smaill (*Associate*), Gp. Capt. C. A. Turner (*Associate Fellow*), Wg. Cdr. W. G. Woodward (*Associate Fellow*).

STATEMENT OF RECEIPTS AND PAYMENTS FOR THE YEAR ENDED 31st DECEMBER 1955

Receipts:	£ s. d.	£ s. d.
Subscriptions, Entrance and Transfer Fees	283 8 8	
Post Office Savings Bank Interest	5 1 0	
		288 9 8
Less Payments:		
Wellington Branch, 50th Anniversary of Flight Expenses	26 2 6	
Postages	9 9 11	
Cable, Telegram and Air Freight Charges	1 9 11	
Stationery	1 11 3	
Bank Fees and Exchange	1 0 6	
		39 14 1
EXCESS OF RECEIPTS OVER PAYMENTS		248 15 7
FUNDS ON HAND 1ST JANUARY 1955		635 15 7
TOTAL FUNDS AT 31ST DECEMBER 1955		£884 11 2

TOTAL FUNDS AT 31ST DECEMBER 1955 COMPRISED:—

Petty Cash	5 4 3
National Bank of New Zealand	374 5 11
Post Office Savings Bank	505 1 0
	£884 11 2

At the 31st December 1955 the sum of £274 16s. 9d. was due for transfer from the New Zealand Division to the Royal Aeronautical Society, London, on account of two years' subscriptions as follows:—

1954 Transfer due	£124 19 0
1955 " "	149 17 9
	£274 16 9

(Sgd.) O. A. KEMP, A.R.A.N.Z., A.C.A.I.,
Hon. Treasurer.

I have audited the accounts of the Royal Aeronautical Society, New Zealand Division, and the above is a true and correct statement of receipts and payments for the year ended 31st December 1955.

(Sgd.) B. W. Hoult, B.Com., A.R.A.N.Z.,
Hon. Auditor.

TECHNICAL NOTES

Contributions to this Section of the JOURNAL *will be eligible for Journal Premium Awards and will normally be published within two months of being received.*

The Compression of Circular Rings

by

W. JOHNSON

(*Department of Mechanical Engineering, University of Sheffield*)

ASSUMING the formation of plastic hinges at the quarter points of a ring subject to a diametral compressive force, a comparison is made between experimental collapse loads and those predicted by use of a simple expression based on the use of plastic rectangular stress blocks, for some common materials; good correlation is obtained with aluminium.

INTRODUCTION

A member may be said to fail when it yields completely at several positions and so allows collapse as a mechanism to occur. The smallest load at which this happens marks the end of useful life of the member. In the case of very thin rings loaded at the ends of a diameter in compression, as shown in Fig. 1, it is obvious that the sections of greatest bending moment are at A, B, C and D. These moments will continue to increase with increasing diametral load. When the material across the whole of the section at each point has become plastic, yielding occurs and deformation as a mechanism can ensue as shown in Fig. 2. The quadrants between the plastic hinges at A, B, C and D are supposed to remain substantially undeformed. Assuming an ideal rigid-perfectly plastic material, *i.e.* one which does not deform or strain when subject to elastic stress but flows or yields without hardening when the elastic stress reaches the yield value, it is clear that hinges will first form at A and C, but significant plastic deformation cannot proceed because the structure cannot become a mechanism, this is only possible when two further hinges have formed at B and D. Before the structure deforms as a mechanism, elastic straining occurs, but once the load is large enough to create all four hinges, the deformation—as measured say by the reduction in diameter in the line of the force—can become large, and the structure can be said to be collapsing. The load when collapse is imminent is called P. For thin rings by this approach, the collapse load is easily calculated[1] but apart from this it is worth while to enquire how far a similar elementary analysis will apply to a ring that is not thin and to materials that do not closely approximate to the ideal one.

1. THE COLLAPSE LOAD

(*a*) *Thin Ring*

For a thin ring loaded as in Fig. 1, P is obtained thus. When the structure has collapsed the work done by the load will be $2Pd$ where d is the distance moved through by P; this energy will be dissipated internally in plastic deformation at the hinges[2]. If M denotes the full plastic bending moment at a hinge and if shear force effects are neglected, then the angle through which each moment rotates is $\sim d/R$, so that

$$2Pd = 8M\,(d/R)$$

and

$$P = 4M/R. \quad . \quad . \quad . \quad . \quad (1)$$

The value of M is easily shown to be $t^2Y/4$ per inch width of ring, where t is the ring thickness and Y the yield stress of the material which is assumed to be the same in tension and compression.

(*b*) *Ring not Thin*

Again assume plastic hinges form at A, B, C and D, then in Fig. 3, AB is a quadrant of the mean radius of the ring, R. Acting at A and B are the moments M_A and M_B and the forces $P/2$. For moment equilibrium on the quadrant

$$PR/2 + (M_A + M_B) = 0 \quad . \quad . \quad (2)$$

and putting $P/2Y = z$, where Y is the yield stress of the material, (2) becomes

$$zR + (M_A + M_B)/Y = 0. \quad . \quad . \quad (3)$$

Across the section at B the rectangular stress blocks are as shown in Fig. 4; and from the stress distribution, for force equilibrium it is found that

$$Yx - Y\,(2a - x) = P/2$$

or

$$x = a + z/2 \quad . \quad . \quad (4)$$

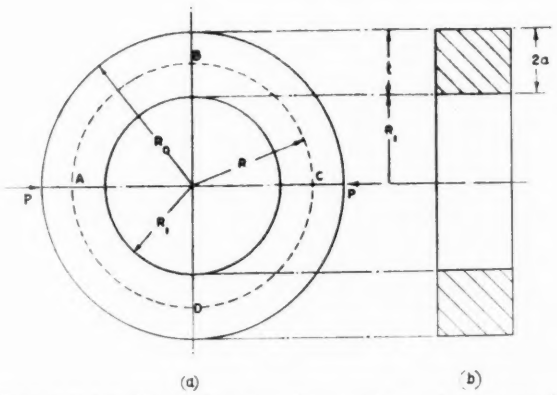

FIGURE 1 (*a*) Ring of rectangular section under diametral compressive load
(*b*) Cross section of ring.

Received 18th April 1956

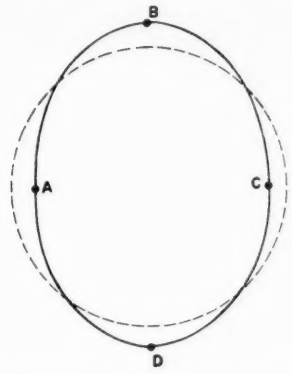

FIGURE 2. Diagrammatic illustration of the collapse of a ring.

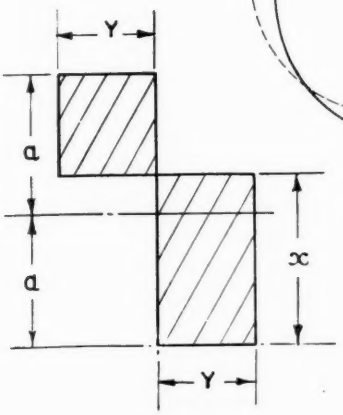

FIGURE 4. Stress blocks at *B*.

and for moment equilibrium, that

$$-Yx(2a-x)=M_B. \quad . \quad . \quad (5)$$

Equation (5) yields on using (4)

$$-M_B/Y=(4a^2-z^2)/4. \quad . \quad . \quad (6)$$

Across section *A*, it is assumed that the bending stress distribution is as shown in Fig. 5. The shear force $P/2$ is taken by the mid-portion of the ring of thickness $2d$, as a uniformly distributed maximum shear stress cY. The existence of elastic bending stress over this central portion is omitted since this together with the yield shear stress would require the material to transmit stresses in excess of its yield strength. Hence at *A* from force equilibrium considerations

$$P/2=2dcY$$

$$\text{or} \qquad d=z/2c \quad . \quad . \quad (7)$$

and for moment equilibrium

$$-Y(a-d)(a+d)/2=M_A/2$$

$$\text{or} \qquad -M_A/Y=a^2-z^2/4c^2 \quad . \quad (8)$$

substituting from (7).

Using (6) and (8) in (3), it is found that

$$bz^2+zR-2a^2=0 \quad . \quad . \quad . \quad (9)$$

where $b=(c^2+1)/4c^2$ and z is now the unknown. Thus from (9)

$$P/RY=[(1+8a^2b/R^2)^{\frac{1}{2}}-1]/b. \quad . \quad . \quad (10)$$

For ductile materials it is well established that $0{\cdot}5<c<\sqrt{3}/3$, the lower value corresponding to Tresca's yield criterion and the upper value to that of von Mises. When $c=0{\cdot}5$, $b=1\frac{1}{4}$ and when $c=\sqrt{3}/3$, $b=1$. For $0\leqslant a/R\leqslant 0{\cdot}5$ for both values of c, Fig. 6 shows the variation of P/RY with a/R.

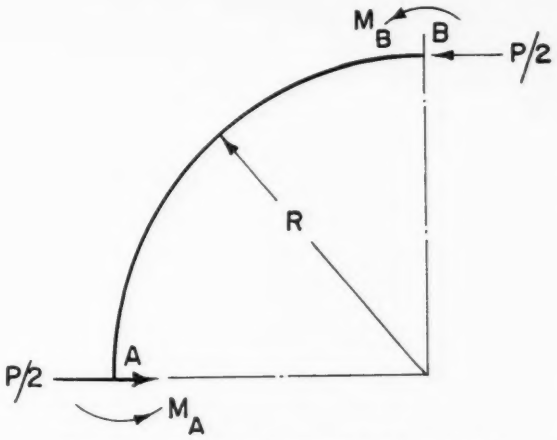

FIGURE 3. Forces and moments on a quadrant.

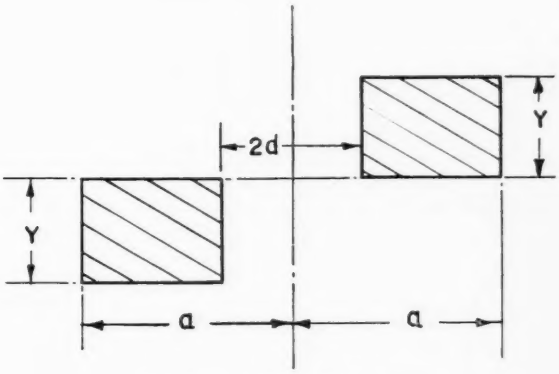

FIGURE 5. Stress blocks at *A*.

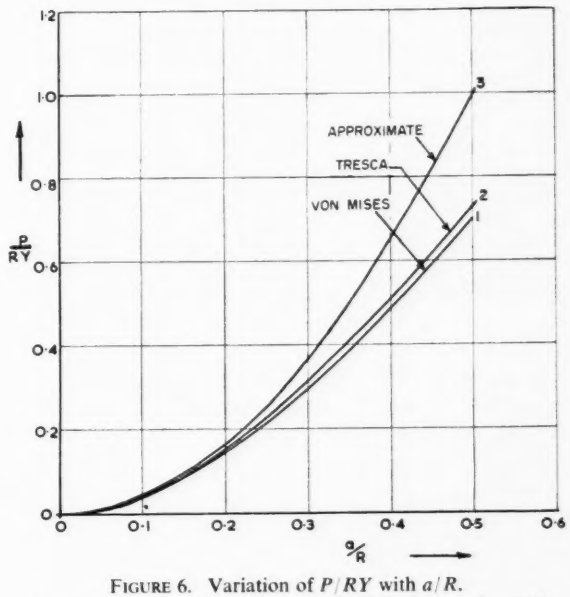

FIGURE 6. Variation of P/RY with a/R.
(1) $c=\sqrt{3}/3$ (2) $c=1/2$ (3) From expression (11)

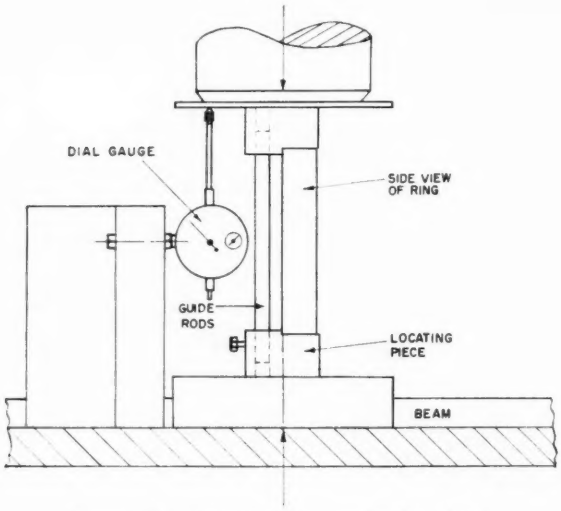

FIGURE 7. Apparatus for compressing rings.

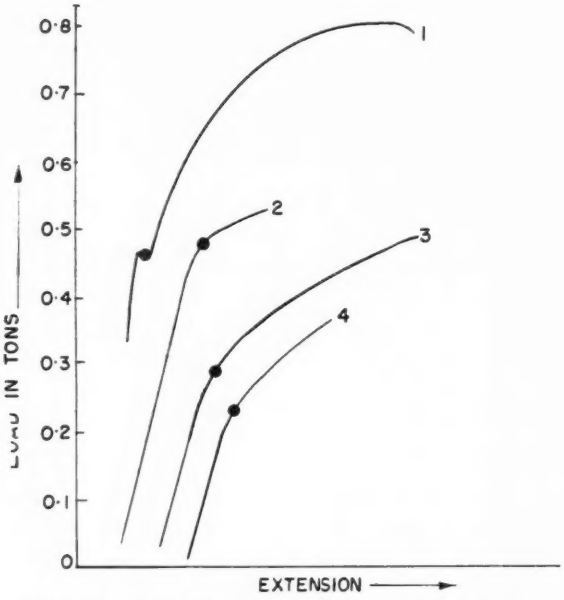

FIGURE 9. Load-Extension curves for materials used

(1) Mild Steel (2) Aluminium
(3) Brass (4) Magnesium Alloy
● Yield Point

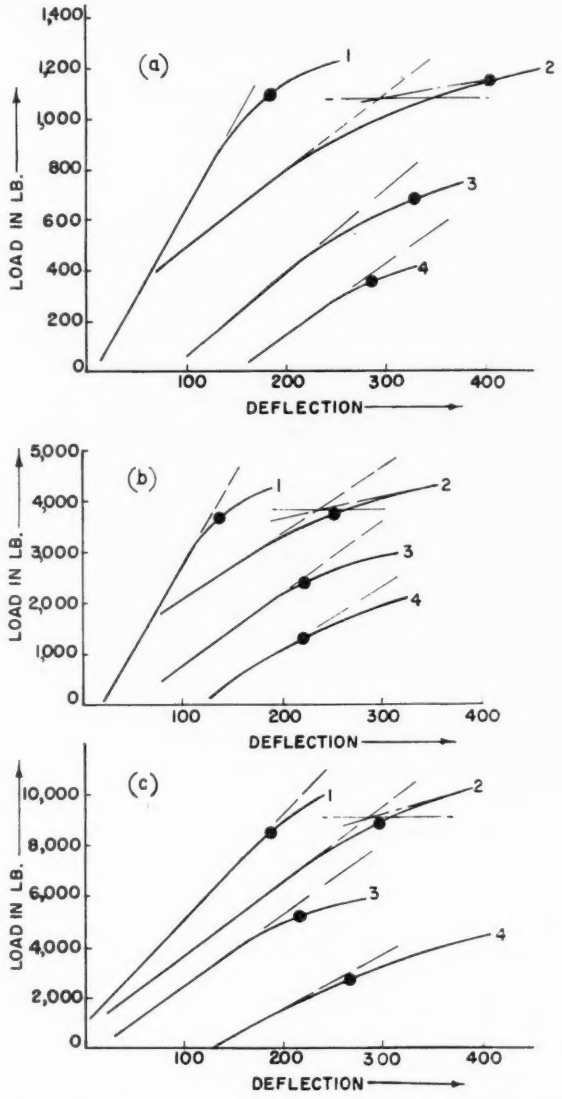

FIGURE 8. Experimental results showing load-deflection curves for rings

(1) Mild Steel (2) Aluminium
(3) Brass (4) Magnesium Alloy
● Predicted value of collapse load

(*a*) $a/R = 0{\cdot}142$ (*b*) $a/R = 0{\cdot}231$ (*c*) $a/R = 0{\cdot}331$

TABLE I

Yield stress in lb./in.^{2}			39,700	41,000	29,700	20,300
R_0 in inches			1·25	1·25	1·25	1·00
R_1/R_0	a/R	P/R_0Y	Mild Steel lb.	Aluminium lb.	Brass lb.	Magnesium Alloy lb.
0·75	0·143	0·08	1100	1140	680	310
0·625	0·229	0·195	3620	3740	2250	1260
0·50	0·331	0·365	8540	8820	5300	2750

FIGURE 10. Photograph showing part of a hinge in process of yielding: dark zones indicate regions of complete plastic deformation

When a/R is small and $b=1$, it will be seen that equation (10) reduces to

$$P/RY = 4a^2/R^2 \quad . \quad . \quad . \quad (11)$$

which is in agreement with (1).

2. EXPERIMENTAL WORK

Experiments were carried out on sets of three rings made of mild steel, hard aluminium and brass (each of 2·5 in. diameter) and magnesium alloy (each of 2·0 in. diameter). The rings were machined from solid bar and were of approximately square section. The range of values of R_i/R_o was 0·75 to 0·5. The apparatus used to perform the tests is shown containing a ring in Fig. 7. The locating pieces can slide vertically along two silver steel guide rods. The lower locating piece remains stationary on the platform of the testing machine while the top piece is made to move downwards and so compel the ring to shorten its vertical diameter. The relative vertical movement of the locating pieces was measured using a common dial gauge.

3. RESULTS

The results of each experiment on a ring—compressive load *versus* diametral deflection—are given in Fig. 8. For each material investigated a tensile load-extension curve was obtained using a small tensile specimen $1\frac{1}{4}$ in. long and 0·180 in. diameter in a Hounsfield tensometer. Each tensile specimen was machined from the portion of a ring between a pair of plastic hinges. The curves for the materials are shown in Fig. 9.

Assuming all the materials to yield according to a Mises criterion, the predicted collapse load for each ring was calculated and these are shown in Table I.

These loads have been inserted on the appropriate load-defection curve for each ring.

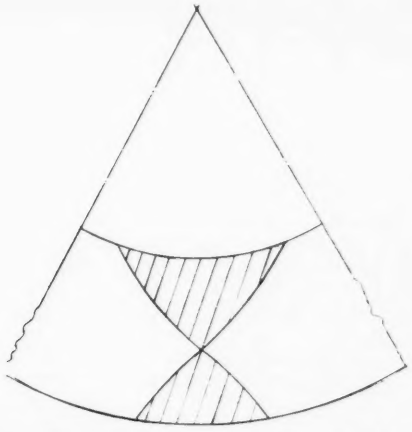

FIGURE 11. Theoretical form of one hinge in plane strain: shaded areas denote regions of plastic deformation

An indication of the extent of one half of the plastic hinge forming in mild steel at the ends of the diameter perpendicular to the load line, is shown in Fig. 10. The dark markings springing from the inner diameter indicate regions of plastically deformed material. This photograph was obtained from a ring which had been highly polished and then strained; only one half of the hinge appeared on photographing it. The actual form of the hinge will be somewhat as shown in Fig. 11[3]. For a ring deformed under conditions of plane strain the shaded areas would be defined by logarithmic spirals of slip lines, this form being indicated by an exact analysis of the problem[3].

CONCLUSIONS

In the case of aluminium there is a large difference between the elastic and plastic moduli, so that strain-hardening effects are small; thus this material most nearly corresponds to that assumed in the usual plastic analyses. The collapse loads predicted for aluminium correspond well with those obtained as the result of the intersection of the elastic and plastic load lines. Similar observations do not apply to the other materials where the strain-hardening rate is not small. With the steel rings reasonable values of the collapse load can be predicted largely because the yield stress of the material is so sharply marked. With brass and magnesium alloy, predicted loads can vary much because the yield stress (or proof stress) to be used as Y in the expression (10), can vary greatly according to how it is chosen.

However the method and expressions derived above can be said to be accurate enough for most practical cases and useful because the analysis and methods are easy and simple to apply.

ACKNOWLEDGMENT

The author's thanks are due to Dr. B. W. Senior for his assistance and useful comments at various points in the work.

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2. GREENBERG, H. J. and PRAGER, W. (1951). *Proc. A.S.C.E.*, Vol. 17, February 1951.
3. JOHNSON, W. Unpublished work.

Buckling of Rectangular Plates with One Unsupported Edge, Compressed by Forces Distributed Along its Edges

by

P. SHULESHKO

(The New South Wales University of Technology)

THE ELASTIC STABILITY of rectangular plates compressed in two perpendicular directions by forces uniformly distributed along the edges is considered. Edges $x=0$ and $x=a$ are simply supported, edge $y=0$ is free and edge $y=b$ is rigidly built-in (Fig. 1). Using the method described in a previous paper[1,2], after satisfying the boundary conditions at the edges of plate, the following transcendental equation is obtained:

$$\alpha\beta\,(A^2+B^2)\cosh\alpha\cos\beta-(\alpha^2B^2-\beta^2A^2)\sinh\alpha\sin\beta-2\alpha\beta AB=0 \quad . \quad . \quad . \quad (1)$$

where

$$\alpha=\lambda\sqrt{\left\{\left(1-\frac{k}{2}\right)+\sqrt{\left[\left(1-\frac{k}{2}\right)^2-(1-\nu k)\right]}\right\}}$$

$$\beta=\lambda\sqrt{-\left(1-\frac{k}{2}\right)+\sqrt{\left[\left(1-\frac{k}{2}\right)^2-(1-\nu k)\right]}\right\}}$$

$$A=\alpha^2-\lambda^2\mu,$$

$$B=\beta^2+\lambda^2\mu,$$

$$\lambda=\frac{mb}{a},\qquad k=\frac{P_2a^2}{m^2\pi^2D},\qquad \nu=\frac{P_1}{P_2}.$$

NOTATION

- D flexural rigidity of the plate $=\dfrac{Eh^3}{12\,(1-\mu^2)}$
- h thickness of plate,
- μ Poisson's ratio
- m whole number of half waves of the buckled plate in the x-direction
- a length of plate
- b width of plate

The expression for the critical force P_{2cr} can be represented in the following form[1,2]

$$P_{2cr}=k\frac{m^2\pi^2D}{a^2} \quad . \quad . \quad . \quad (2)$$

where k is a non-dimensional coefficient, and is the smallest root of equation (1).

The value of k depends on the magnitude of the ratio mb/a of Poisson's ratio μ, and of the ratio ν.

The following three cases of the ratio ν are considered: (*i*) $\nu=1$, (*ii*) $\nu=0$, and (*iii*) $\nu=\infty$, or $\nu_1=0$ where

$$\nu_1=\frac{P_2}{P_1}.$$

Poisson's ratio μ for all cases is taken equal to 0·30. The values of k are given in Figs. 1, 2 and 3.

Case when $\nu=1$

The curve $a_1b_1c_1$ in Fig. 1 corresponds to the first mode of buckling of the middle plane of the plate (Fig. 1*b*) when $\nu=1$. The curve $a_2b_2c_2$ in Fig. 1 corresponds to the second mode of buckling (Fig. 1*c*) of the plate when $\nu=1$. The curves which correspond to the third, fourth, and higher modes, although not shown in the graph, could form a natural sequence and could be shown one above the other. The curve $a_0b_0c_0$, which is shown in the same graph, corresponds to the first mode of buckling of the plate with simply-supported edges when $\nu=1$, and is given for comparison.

Case when $\nu=0$

The highest curve in Fig. 1 corresponds to the first mode of buckling of the plate when $\nu=0$.

Any ν *from interval* $0\leqslant\nu\leqslant1$

The intermediate values of k for different values of ν from the interval $0\leqslant\nu\leqslant1$ will have corresponding curves for different values of ν which will be placed between the highest curve ($\nu=0$) and the lowest curve ($\nu=1$) of Fig. 1.

Long Plates $a>b$

For long plates with $a>b$ it is handy to use $k'=\dfrac{P_2b^2}{m^2\pi^2D}$ instead of k. The smallest values of k' are also represented by curves in Fig. 2. The intermediate value of k', as before, will be located between the highest ($\nu=0$) and lowest ($\nu=1$) curves in Fig. 2. The lowest curve at $a/b\longrightarrow\infty$ approaches the straight line corresponding to the value $k'=\frac{1}{4}$ for the long plate.

Case when $\nu=\infty$, *or* $\nu_1=0$

The smallest roots of $k_1=\dfrac{P_1a^2}{m^2\pi^2D}$ for the case when $\nu=\infty$, *i.e.* $P_2=0$, *or* $\nu_1=P_2/P_1=0$ are represented by the highest curve in Fig. 3. The lowest curve in Fig. 3 corresponds to the values of k_1 when $\nu=\nu_1=1$.

Any ν_1 *from interval* $0\leqslant\nu_1\leqslant1$

The intermediate values of k_1 for different values of ν_1 from the interval $0\leqslant\nu_1\leqslant1$ or different values of ν from the interval $\infty\geqslant\nu\geqslant1$ will be represented by curves (not shown in Fig. 3) placed between the curves for $\nu_1=1$ (or $\nu=1$) and $\nu_1=0$ (or $\nu=\infty$).

CONCLUSION

From the cases of the buckling of plates considered above it follows that if $\nu=1$ and $mb/a\geqslant2{\cdot}06$ all roots of k can be taken equal to one ($k=1$); if $\nu=0$ and $mb/a\geqslant1{\cdot}80$ all roots of k can be taken equal to 2·31 ($k=2{\cdot}31$). If $\nu_1=1$, $a/(mb)\longrightarrow\infty$ roots of k' approached 0·25; and if $\nu_1=0$, $a/(mb)\longrightarrow\infty$ roots of k' approach 1·33.

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1. SHULESHKO, P. (1948). Stability of Thin Plates which have Part of the Contour Unsupported. Scientific Memoirs of Ukrainian Technical University at Regensburg, Germany (in Ukrainian), 1948.
2. SHULESHKO, P. (1953). Buckling of Rectangular Plates Uniformly Compressed in the Vertical Direction with Upper Edge Free. *Journal of the Institution of Engineers, Australia.* Vol. 25, No. 4-5, 1953.

Received 20th March 1956

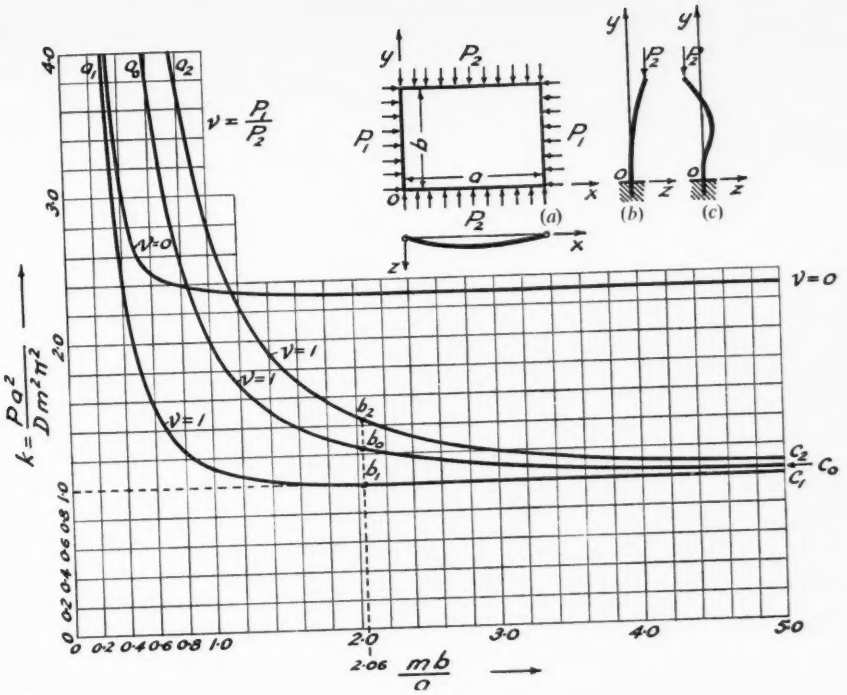

FIGURE 1.

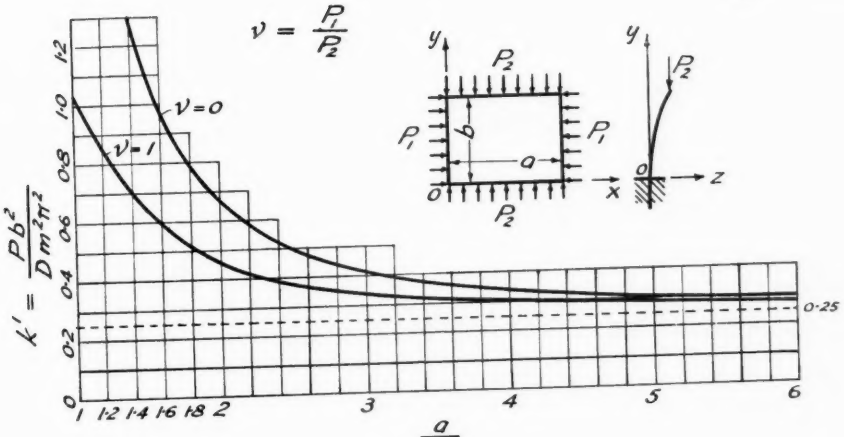

FIGURE 2.

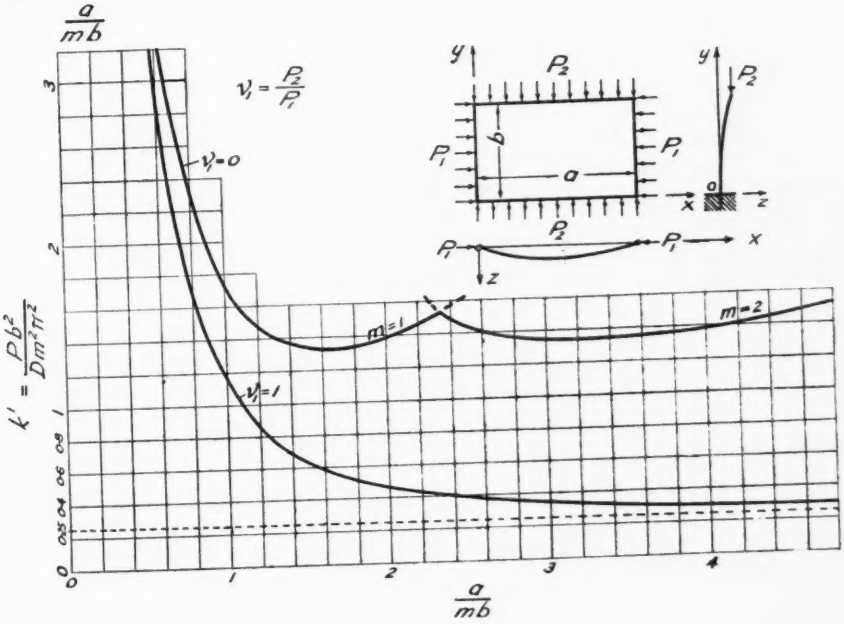

FIGURE 3.

Reciprocation of Triply Partitioned Matrices

by

E. KOSKO

(Avro Aircraft Limited, Malton, Canada)

IN A RECENT NOTE[1] Professor Duncan gives a method for finding the reciprocal of a matrix which is triply partitioned both horizontally and vertically. In spite of the apparent complication of the formulae, such a procedure may shorten the calculations in the case when the submatrices involved are especially simple (*e.g.* null or diagonal), or when their arrangement exhibits some symmetry. In Ref. 1, the submatrices a to j of the desired reciprocal are obtained by a series of operations summarised in equations (3) to (19); these operations consist altogether of 5 inversions of submatrices, 29 matrix multiplications of various orders and 15 matrix additions.

The author proposes to show how, by applying a method presented in Ref. 2, the algorithm can be simplified and put in a more schematic form. In its essence, the method consists in reducing the given matrix to a matrix in which all the submatrices outside the principal diagonal are null, *i.e.* to a so-called direct sum. The latter has for its reciprocal the direct sum of the reciprocals of its submatrices. The reduction is performed by pre-multiplying and post-multiplying the given matrix by suitable non-singular matrices; in most cases these matrices can be readily obtained from the given matrix.

To make comparisons easier, Professor Duncan's notation is retained as far as possible. The submatrices of the original matrix U shall be denoted by A to J, those of the reciprocal by a to j, and auxiliary matrices by K to P. The auxiliary matrices R and S are not required; instead, two additional auxiliary matrices are defined as

$$T = G - HK^{-1}Q, \qquad . \quad . \quad . \quad (1)$$

$$W = J^{-1} + J^{-1}HK^{-1}FJ^{-1}. \qquad . \quad . \quad (2)$$

The reduction mentioned above is represented by a relation

$$V = XUY \qquad . \quad . \quad . \quad (3)$$

where U is the given matrix, V is the " direct sum," X and Y the pre-multiplying and post-multiplying matrices, respectively. The reciprocal of U is obtained from the relation

$$U^{-1} = YV^{-1}X, \qquad . \quad . \quad (4)$$

which is a consequence of (3). In our case, we must put

$$X = \begin{bmatrix} I_r & -LK^{-1} & -MJ^{-1} \\ 0 & I_s & -FJ^{-1} \\ 0 & 0 & I_t \end{bmatrix}, \qquad Y = \begin{bmatrix} I_r & 0 & 0 \\ -K^{-1}Q & I_s & 0 \\ -J^{-1}T - T^{-1}H & 0 & I_t \end{bmatrix} \qquad . \quad . \quad . \quad (5)$$

As can be easily verified by substituting these into equation (3) and performing the multiplications, the reduced matrix can be written as

$$V = \begin{bmatrix} N & 0 & 0 \\ 0 & K & 0 \\ 0 & 0 & J \end{bmatrix}, \qquad . \quad . \quad . \quad (6)$$

and its reciprocal as

$$V^{-1} = \begin{bmatrix} N^{-1} & 0 & 0 \\ 0 & K^{-1} & 0 \\ 0 & 0 & J^{-1} \end{bmatrix}. \qquad . \quad . \quad . \quad (7)$$

Substitution of this into equation (4) yields the desired reciprocal.

The explicit expressions for the submatrices a, b and c can be seen as identical with those of Ref. 1; for the remaining ones we obtain

$$d = -K^{-1}QN^{-1}, \qquad . \quad . \quad . \quad . \quad (8)$$

$$e = K^{-1} - dLK^{-1}, \qquad . \quad . \quad . \quad . \quad (9)$$

$$f = -K^{-1}(F - QN^{-1}M)J^{-1}, \qquad . \quad (10)$$

$$g = -J^{-1}TN^{-1}, \qquad . \quad . \quad . \quad . \quad (11)$$

$$h = -J^{-1}(H - TN^{-1}L)K^{-1}, \qquad . \quad . \quad (12)$$

$$j = W + J^{-1}TN^{-1}MJ^{-1}. \qquad . \quad . \quad . \quad (13)$$

Having been obtained by direct substitution, these expressions require no further proof. The only condition for the validity of the procedure is that none of the matrices J, K or N be singular.

The author has found it more practical to arrange the calculations according to a schematic lay-out. The various intermediate matrices are aligned in two auxiliary arrays; in these the order of each matrix is the same as that of a similarly situated submatrix of the original matrix U. The calculations proceed by a series of simple steps, each step being either an inversion, a matrix mutiplication, or a matrix multiplication followed by an addition. After some experience with these operations, the writing down of the expressions such as (8) to (13) becomes unnecessary.

The first auxiliary array is derived from the given matrix as follows,

$$\begin{bmatrix} \bar{A} & \bar{B} & \bar{C} \\ \bar{D} & \bar{E} & \bar{F} \\ \bar{G} & \bar{H} & \bar{J} \end{bmatrix} = \begin{bmatrix} A - CJ^{-1}G & B - CJ^{-1}H & -CJ^{-1} \\ D - FJ^{-1}G & E - FJ^{-1}H & -FJ^{-1} \\ -J^{-1}G & -J^{-1}H & J^{-1} \end{bmatrix} \qquad (14)$$

First, the reciprocal of J is computed, then $\bar{C}$, $\bar{F}$, $\bar{G}$ and $\bar{H}$ are obtained each by a matrix multiplication; finally, $\bar{A}$, $\bar{B}$, $\bar{D}$ and $\bar{E}$ are computed from the preceding ones each by a matrix multiplication and an addition. It will be noted

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that the latter four matrices are identical with P, L, Q and K, respectively, of Ref. 1. If necessary the triple products may be computed in two different ways for checking purposes.

The second auxiliary array is then obtained in a similar way from the first one:

$$\begin{bmatrix} \overline{\overline{A}} & \overline{\overline{B}} & \overline{\overline{C}} \\ \overline{\overline{D}} & \overline{\overline{E}} & \overline{\overline{F}} \\ \overline{\overline{G}} & \overline{\overline{H}} & \overline{\overline{J}} \end{bmatrix} = \begin{bmatrix} \overline{A} - \overline{B}\overline{E}^{-1}\overline{D} & -\overline{B}\overline{E}^{-1} & \overline{C} - \overline{B}\overline{E}^{-1}\overline{F} \\ -\overline{E}^{-1}\overline{D} & \overline{E}^{-1} & +\overline{E}^{-1}\overline{F} \\ \overline{G} - \overline{H}\overline{E}^{-1}\overline{D} & +\overline{H}\overline{E}^{-1} & \overline{J} + \overline{H}\overline{E}^{-1}\overline{F} \end{bmatrix} \quad (15)$$

Here, the computation begins with the inversion of $\overline{E}$; then $\overline{\overline{B}}$, $\overline{\overline{D}}$, $\overline{\overline{F}}$ and $\overline{\overline{H}}$ are obtained each by a matrix multiplication; finally, $\overline{\overline{A}}$, $\overline{\overline{C}}$, $\overline{\overline{G}}$ and $\overline{\overline{J}}$ are found from the previous ones, each by a matrix multiplication and an addition. Incidentally, the notations for the last four matrices are equivalent to N, $-MJ^{-1}$, $-J^{-1}T$ and W respectively, used in the first part of this note. The occurrence of plus and minus signs must be carefully observed.

In a third and final series of steps, the sought reciprocal is found from the second auxiliary array in the following way:

$$U^{-1} = \begin{bmatrix} a & b & c \\ d & e & f \\ g & h & j \end{bmatrix} = \begin{bmatrix} \overline{\overline{A}}^{-1} & \overline{\overline{A}}^{-1}\overline{\overline{B}} & \overline{\overline{A}}^{-1}\overline{\overline{C}} \\ \overline{\overline{D}}\overline{\overline{A}}^{-1} & \overline{\overline{E}} + \overline{\overline{D}}\overline{\overline{A}}^{-1}\overline{\overline{B}} & \overline{\overline{F}} + \overline{\overline{D}}\overline{\overline{A}}^{-1}\overline{\overline{C}} \\ \overline{\overline{G}}\overline{\overline{A}}^{-1} & \overline{\overline{H}} + \overline{\overline{G}}\overline{\overline{A}}^{-1}\overline{\overline{B}} & \overline{\overline{J}} + \overline{\overline{G}}\overline{\overline{A}}^{-1}\overline{\overline{C}} \end{bmatrix} \quad (16)$$

The operations here begin with the reciprocation of $\overline{\overline{A}}$, proceed with obtaining b, c, d and g each by a matrix multiplication, and then e, f, h and j each by a matrix multiplication and an addition.

The arrays (14) and (15) are not to be interpreted as matrices in the usual sense. They are merely convenient arrangements of all the intermediate matrices involved, and at the same time they present a schematic plan of operations.

The count of operations is now as follows: 3 inversions (of orders t, s and r); 24 matrix multiplications and 12 additions or subtractions. This total represents a worthwhile saving as compared to Ref. 1. Moreover, the symmetry of the arrangement permits various short-cuts when dealing with symmetrical matrices. Thus, in the case when the submatrices D, G and H are equal to the transposed matrices B, C and F, respectively, the number of matrix multiplications is reduced to 15, and the number of additions to 9.

In Ref. 2, the author has demonstrated the application of the same principles to many other cases of partitioning. Due to lack of space, discussion of the reciprocation of a general triply partitioned matrix was not included there.

REFERENCES

1. DUNCAN, W. J. (1956). Reciprocation of Triply Partitioned Matrices. *Journal of the Royal Aeronautical Society*, Vol. 60, p. 131, February 1956.

2. KOSKO, E. Matrix Inversion by Partitioning. To be published in *The Aeronautical Quarterly*.

Determination of Velocity and Pressure Distribution along the Surface of Annular Aerofoils with Thick Symmetrical Section

by

S. PIVKO

(Aerotechnical Institute, Beograd, Yugoslavia)

IN my Technical Note published in the May 1956 JOURNAL (pp. 348-350), equation (2) gives the total strength of a single source ring. The velocity components v_{qr}* and v_{qr}*, tabulated by Küchemann and Weber, however, were obtained by integration round the circumference assuming a distribution of sources of constant strength along a circle. Hence, the right hand sides of equations (5) and (6) are too large by a factor 2π. This produces the factor 2π in equation (12). The result of numerical calculations would thus be that the velocity on an annular aerofoil is to a first order equal to that on the corresponding two-dimensional aerofoil, as is to be expected for small values of l/R. The deviation is, of course, the greater the larger the ratio l/R and the larger the thickness-chord ratio of the aerofoil. This implies that further singularities, *e.g.* a vortex ring distribution, are necessary in addition to the source ring distribution, to represent an annular aerofoil with symmetrical section shape.

I am indebted to Mrs. J. Weber, Aero Department, Royal Aircraft Establishment, South Farnborough, for pointing out this fact.

Graduates' and Students' Section

This page of the Journal is written by members of the Committee of the Graduates' and Students' Section. Each month it covers matters of interest and importance to Graduates and Students and previews lectures and visits.

IN THE MAY JOURNAL Mr. A. V. Cleaver, F.R.Ae.S., made some interesting and provocative remarks about the outlook of young aeronautical engineers. This month we publish a letter from Mr. F. Brookesmith, A.R.Ae.S., who puts a viewpoint of, in his words, "The somewhat older generation." He writes:—

"While the present better conditions are wholly good and to be encouraged, there is probably a lack of appreciation by the young men of today of how much better off they are than their fathers were and of how those fathers did a great deal to bring about these conditions. These gains were not made by agitation and rebellion but by application to the job in hand in spite of long hours, low pay and poor working arrangements. It does take a generation or two for these qualities to be appreciated but the well-equipped, well-lit drawing offices of today, the shorter hours and better pay are the results of the work of that older generation who pressed on and laughed off the difficulties to do a job because of interest, enthusiasm and pride in their work.

"I do not suggest that these qualities do not exist today. They do. I do not believe that engineers, like everybody else, are very different from what they always were. The grousers have always been, and always will be with us, and the enthusiasts too.

"The point I wish to make is that the present-day engineer can show his gratitude for the facilities he enjoys by putting out a much greater effort for the hours he has to work. He is under less pressure than his forbears. It has probably always been true for some to say that a sense of urgency is lacking. But removal of the fear of loss of employment may have made the lack more general. The country's economic and scientific position has made the need for this sense of urgency even greater if we are to keep our position among the nations."

[Further correspondence on this subject will be welcomed. Students and graduates have views of their own; may we hear them, please?]

VISITS

U.S.A.F. Base at Upper Heyford

On the afternoon of Friday 18th May, some 35 members travelled by coach and car to the base to see the aircraft to be shown to the public on U.S. Armed Forces Day. The Section's particular wish, to see in detail a Boeing B-47 was well fulfilled, and one such aircraft was parked at our disposal for some two hours, during which time three flying crew members answered dozens of our questions, and most of the party were able to climb into the cockpit.

We were impressed by the size and complexity of this machine. The famous flexible wing vibrated noticeably in the wind. There were numerous interesting features of the construction and services, which made the inspection of considerable educational value.

Since the crew consists of only three, each has much to do, and it is all the more remarkable that although primarily either pilot or navigator, each man has to be fully conversant with *all* the aeroplane and its equipment, so as to be interchangeable in emergency. They also have to have 2,500 flying hours before flying a B-47.

The station was operational on the day, and the U.S. Strategic Air Command is well-known for the stringency of its security arrangements. A Boeing KC-97 flying tanker aircraft parked nearby had an armed guard, and there was a line-up of British types including a Hunter, which we were not allowed to go near, because official permission had not been granted. Before we left, an F-86D all-weather fighter landed, and we enjoyed a brief inspection of this aircraft and conversation with its enthusiastic pilot.

The visit was slightly disappointing, in that we were unable to see all the types that were promised verbally, but the B-47 is worth going a long way to see.

Handley Page, Ltd.

On Saturday morning, 9th June, a somewhat depleted party of about 25 out of 30 applicants arrived at the Cricklewood works where a tour lasting about an hour was made of the workshops and layout department. In the former, something was seen of the metal corrugated sandwich panel manufacture used extensively in the construction of the Victor, the machine shop where some machining from thick plate was in progress, and the large spot-welding shop.

The party then travelled to Radlett where morning coffee was served before the Press Shop was visited. In here, there is a large rubber press, and several drop hammers. A short visit was also made to the Victor Servicing School before moving on to Park Street.

The test department is at the other end of the aerodrome. In the flight sheds there, we gained an external view of the Victor prototype and three production aircraft. Also present were the Herald prototype and the Vampire laminar-flow research aircraft, but the latter's suction installation was covered up. The Victor prototype was probably the most interesting, in view of the test installations and instrumentation to be seen. In the structural test house, the main frame of 400 tons loading capacity contained a test specimen comprising almost an entire Victor wing and fuselage. Smaller frames were set up with a Herald fuselage and a Victor tail unit. A systems test rig enabled searching tests to be made on the power controls, auto-pilot and air-conditioning systems.

BOSCOMBE DOWN

We are trying to arrange a weekday visit to the Aircraft and Armament Experimental Establishment at Boscombe Down, but it cannot take place in July, and we are now hoping to obtain a date in the last week of August. Will members wishing to attend a visit to Boscombe Down write to the Hon. Visits Secretary, Mr. N. K. Benson, 14 Wakering Road, Barking, Essex, stating their grade of membership and nationality.

THE LIBRARY

Reviews

SURVEYS IN MECHANICS. *Edited by G. K. Batchelor and R. M. Davies. Cambridge University Press, 1956. 475 pp. Illustrated. 50s.*

This interesting volume carries on its title page the inscription " A collection of surveys of the present position of research in some branches of Mechanics, written in commemoration of the seventieth birthday of Geoffrey Ingram Taylor," and forms a very delightful tribute to a great mathematical physicist of our time, whom we all admire and whose work has shed great light on the most diverse problems. An idea of the nature of the book can be given by quoting its contents. It opens with a short biographical sketch by Sir Richard Southwell followed by chapters on various branches of Mechanics in which Sir Geoffrey has distinguished himself. These are: The Mechanics of Quasi-Static Plastic Deformation in Metals, by R. Hill. Dislocations in Crystalline Solids, by N. F. Mott. Stress Waves in Solids, by R. M. Davies. Rotating Fluids, by H. B. Squire. The Mechanics of Drops and Bubbles, by W. R. Lane and H. L. Green. Wave Generation by Wind, by F. Ursell. Viscosity Effects in Sound Waves of Finite Amplitude, by M. J. Lighthill. Turbulent Diffusion, by G. K. Batchelor and A. A. Townsend. Atmospheric Turbulence, by T. H. Ellison. The Mechanics of Sailing Ships and Yachts, by K. S. M. Davidson.

Each chapter gives a survey of past research and present knowledge of its subject, and takes care to show clearly where Taylor's work has illuminated the matter under discussion. There is also an extensive bibliography at the end of each chapter. It is not surprising, to those who know Taylor well, to find that in many cases it was he who, by his almost uncanny grasp of fundamentals, opened up new fields of research that subsequently had many followers. Extensive though the field covered by the book is, it by no means includes the whole range of activities in which Sir Geoffrey has been interested. Even so, a versatility, rare in these days of specialisation, is revealed, and those who have not the pleasure of a close personal acquaintance with Taylor's work will be able to see how great has been his contribution to knowledge of the subjects treated.

Sir Richard Southwell's introduction is an intimate pen-portrait of the man, as well as a statement of many of his pre-occupations. Taylor's wisdom and penetration are well known to those who have had the joy of knowing him and working with him. One has so often been struck by the way in which, when serving on a research committee, he would say very little but when he did speak, his words, often far from fully understood at the time, nearly always contained some idea of vital importance to the subject and frequently led to a new development in research. It is not often that a great scientist receives such a graceful tribute to his work as this book represents, and much credit is due to the editors and all others who have obviously taken great pains to make the volume as perfect as possible. We can be quite sure that Sir Geoffrey Taylor will appreciate the compliment to the full, and we can all hope that he will enjoy many years of activity yet, and be able to contribute still further, in his own inimitable way, to the ever more difficult problems which we have to solve.—E. F. RELF.

INTRODUCTION TO PLASTICITY. *Aris Phillips. Ronald Press Co., New York, 1956. 230 pp. Diagrams. $7.*

The title of this book is misleading. It is not an introduction to the plastic flow of metals in the broad sense but only to a restricted field of application in which the mathematics is particularly simple, namely the one-dimensional theory of bending of beams and the limit design of elementary structures. Since the first of these subjects is the author's especial interest, it is not surprising to find that he allows himself the licence of forgetting his title and going into considerable detail for many different shapes of section and types of loading. Indeed, in places, the book becomes a work of reference with lengthy tables of collected formulae.

In the later Chapters the author tries to justify his title by brief incursions into topics such as plane plastic strain, torsion, and thick tubes under pressure. Here, however, he is apparently on unfamiliar ground and is content with perfunctory accounts and inadequate references to the literature, and sometimes also with clumsy and unconsidered analytic treatments (despite the fact that these topics have all been well treated in existing books and papers). An extraordinary piece of " mathematics " appears on pp. 161-3 where the author ties himself in grotesque knots in arriving at an expression (equation 8.111) that could be obtained in one line with the use of Mohr's circle (how curious that this is not used in a book aimed at engineers!)

Mathematicians are often taken to task (sometimes rightly) by engineers and others for making simplifying assumptions without discussing their limitations in relation to actual materials. Yet here is a book written by an engineer for students and practising engineers and one often looks in vain for necessary qualifications of bald statements. To mention the most important instances: recent work on plastic design has revealed clear and important limitations on its validity; the analysis given for wire-drawing and extrusion is a very poor approximation in the practical range and has been long since superseded; the neglect of compressibility in the theory of autofrettage is inadequate in a calculation of the distortion; the points plotted in the Figure giving the results of the classical Taylor-Quinney experiments do *not* coincide with the start of permanent strain (as stated) but are obtained by a backward extrapolation of the stress-strain curve (the reason should certainly be explained to engineering students as it is absolutely fundamental for the expectation and interpretation of discrepancies between fact and theory).

For these reasons the book cannot be recommended as an " Introduction "; it will probably be most useful as a work of reference in connection with plastic bending of beams.—R. HILL.

THEORY OF COMBUSTION INSTABILITY IN LIQUID PROPELLANT ROCKET MOTORS. *Luigi Crocco and Sin I Cheng. A.G.A.R.D. Butterworth, London, 1956. 200 pp. Illustrated. 37s. 6d.*

In contrast to gas turbine practice where " instability " connotes a tendency for the flame to be extinguished. combustion instability in rocket motors is a very violent type of combustion indeed: pressures and flows throughout the supply lines, combustion chamber, and exhaust

system exhibit oscillations of large amplitude and common frequency. The latter feature distinguishes instability from mere "roughness" of burning which is always present to some extent and involves random fluctuations with little correlation from one point to another. Rocket motor instability is essentially the same phenomenon as the "howling" which sometimes affects aircraft engine combustion systems and after-burners. The high stresses and heat transfer rates accompanying oscillating combustion nearly always cause damage to the plant.

The task of predicting, and so avoiding, the onset of instability is made difficult by the fact that we do not yet know enough of conditions inside a rocket motor even in the steady state: the high temperature makes experimental probing almost impossible; the theoretical approach is equally so because we lack knowledge of the aerodynamics of two-phase flow and of the constants of the chemical reactions. It would seem therefore that the transient phenomenon of instability is even more beyond our reach. However the study of highly idealised theoretical models can yield some useful information, and it is such a study that is the subject of Crocco and Cheng's monograph.

Their first simplification is to assume that the chemical reaction process can be represented by the statement that each particle of fuel waits unchanged for a finite time, the "time lag", after injection into the combustion chamber, and then is suddenly transformed into combustion products. The "time lag" is regarded as a characteristic of the fuel, dependent chiefly on chamber pressure. Although this description is a crude approximation, it has a rough truth and enables the theoretical analysis to begin.

The "time lag" concept, together with neglect of non-uniformities within the combustion chamber and within the fuel supply lines, already enables two mechanisms of oscillation to be discussed. Both processes, which occur at relatively low frequencies (20 to 100 cycles / sec.), are known as "chugging." The first mechanism results from the fact that increased fuel flow causes increased chamber pressure which in turn tends to decrease fuel flow; because of the finite "time lag" the adjustment of chamber pressure to a steady value sometimes fails to take place and the system "hunts." The second mechanism is independent of changes of fuel injection rate and occurs because the "time lag" itself decreases as pressure increases; if the pressure exponent is sufficiently great, instability can occur.

High frequency oscillations (500 to 2000 cycles / sec.) also plague the rocket operator. Technically known as "screaming," the process requires a more sophisticated analysis, since non-uniformities of pressure in the combustion chamber and fuel lines are simultaneously the result and the cause of the oscillations. The authors introduce the idea of "space lag" to account for the non-uniformities, and are led into discussion of the time of pressure wave travel.

The major part of the book is taken up by a mathematical discussion of the transient behaviour of the above idealised models, to which various qualifying features are added as needed or desired. The concepts and techniques used are those which have been developed in connection with automatic control theory. One interesting device is the use of the so-called Satche Diagram for control problems with time lag.

A final chapter discusses the relation between experiments and theory. Since the former concern fully-developed instability while the latter concerns incipient instability, the authors disclaim expectation of close agreement, which in any case is precluded by the crudity of the theoretical model. Nevertheless rough agreement is found as to the existence and frequency of oscillation as influenced by various design variables. Although the day is still far off when the stability of a new design can be ensured from the outset, the monograph shows that definite progress has been made.

Like many contributors in the A.G.A.R.D. publication series, the authors are associates of Dr. von Kármán, and chief attention is paid to the work of the groups at Princeton and California. Although their monograph does not contain rules to tell the rocket motor designers what to do or not to do, the research worker in the field will be grateful for this connected disclosure of recent American work.—D. B. SPALDING.

TRAGFLÜGELTHEORIE — INKOMPRESSIBLE FLÜSSIGKEITEN (*Aerofoil Theory—Incompressible Fluids*). *Elie Carafoli. Translated from the Roumanian into German. V. E. B. Verlag Technik. Berlin 1954. 562 pp. Illustrated.*

This official textbook for use at East-German universities and technical colleges is, indeed, an excellent treatise written by an eminent theoretical aerodynamicist who for 30 years or so has been in a key position at the Aerodynamic Institute of St. Cyr, before returning to his native Roumania. While being admirable as a textbook, by an expert who is not only capable of expressing himself in a most lucid manner but who is also aware of what the practical aerodynamicist needs, it is obvious that the author is unaware of much important work which has been done during and after the war; this is also illustrated by the lack of references to British, American and German research work in theoretical aerodynamics during the past 15 years or so, and the foreword provided by the German publisher seems to proffer some sort of apology for this gap. Thus, as far as, for example, the treatment of swept-wing systems, low aspect ratio wings and more recent work on aerofoil-section theory and so on, is concerned, the textbook is not what it might have been if the author had had access to publications of the Western side.

A weakness of the book, particularly for the student, is the omission of practical examples, and the general absence of numerical values.

The first chapters deal with classical hydrodynamics for aeronautical purposes to which the author has substantially contributed in the past (see, for example, the Carafoli sections with rounded trailing edge and other original contributions in the book). Subjects dealt with include: the Kutta-Joukowski theory, and the development of aerofoil sections (including laminar flow profiles); monoplanes and biplanes of finite and of infinite span, with the strict theory of the induced drag, and a discussion of lift-grading calculation methods (to which the author contributes a general method of his own); the theory of "deformed" wings, *i.e.* control surfaces, wing deflections and rotations; the lifting-vortex theory; non-steady lift generation. Multiplanes, aerofoil grids, diffusers and other items of interest for the power plant aerodynamicist are not considered. A substantial and excellent chapter is devoted to flow-separating boundaries at composite lift-generating systems, *i.e.* to the aerodynamic theory of the complete aeroplane. A final chapter refers to applications of the vortex theory to wind tunnel investigations, notably to the corrections of test results.

The German translation and the printing are done with care, and the bibliographic completeness of Russian publications is admirable.—A. R. WEYL.

PLANE WAVES AND SPHERICAL MEANS APPLIED TO PARTIAL DIFFERENTIAL EQUATIONS. *Fritz John. Interscience Tracts in Pure and Applied Mathematics, No. 2. Interscience Publishers, New York, 1955. 172 pp. 32s.*

This tract contains a mixed collection of results on partial differential equations. The unifying element is the use of certain elementary identities for plane and spherical integrals of an arbitrary function. The author shows how a variety of results on fairly general differential equations follows from these identities.

METHODS OF MATHEMATICAL PHYSICS. *Sir Harold Jeffreys and Bertha Swirles. Cambridge University Press, 1956. 714 pp. 84s. Third Edition.*

This is the third edition of a book first published in 1946, but it has not been mentioned in *The Journal* before. The book is already well known and in this new edition changes have been made in four of the chapters. In Chapter 1 on "The Real Variable," some of the theorems have been stated more explicitly, a few proofs have been added and some have been shortened. In Chapter 6 the proof of Poisson's equation has been improved. In Chapter 17 the Airy integral for complex argument is discussed in more detail and conditions are given for uniformity of approximation for asymptotic solutions of Green's type for complex argument. In Chapter 23 some remarks on the analytic continuation of the solutions have been added, and a note applies them to the parabolic cylinder functions.

WELTRAUMSTRAHLUNG (COSMIC RADIATION). *Dr. Jakob Eugster. Hans Huber, Berne, 1955. 141 pp. Illustrated. 26 Swiss francs. (In German).*

All living things receive radiation. In the sphere of Biophysics, many revolutionary developments have occurred in the past few years and our knowledge of the biological effects of cosmic radiation has thereby become widened. Prof. Dr. Jakob Eugster, who has been concerned with the problems of cosmic radiation for the past 25 years, describes briefly the latest results of his research in this volume.

PRACTICAL METROLOGY. VOLUMES 1 AND 2. *K. J. Hume and G. H. Sharp. Macdonald, London, 1953-1956. 60 pp. and 70 pp. Illustrated. 6s. and 7s. 6d.*

This book is to be published in four volumes and will provide collections of laboratory experiments in Metrology with instructions for their performance, recording and the assessment of results. All the experiments described correspond to actual examples encountered in industrial metrology work.

Volume 1 contains experiments on the measurement of plain plug gauges, gap gauges, height of spigots, flush-pin gauges, plain ring gauges, taper plug gauges, taper ring gauges, the checking of a straight-edge and an engineer's square and the calibration of a micrometer.

Volume 2 contains experiments on the measurement of plug gauges, angle of taper plug gauges, plate gauges, spur gears, plug screw gauges, pitch error of screw gauges, forms and angles of plug screw gauges, minor diameters of plug screw gauges, major, minor and effective diameters of ring screw gauges, the checking of a form gauge and a sine bar and taking a cast of a small ring screw gauge.

THE OBSERVER'S BOOK OF AIRCRAFT. *William Green and Gerald Pollinger. Frederick Warne, London, 1955. 287 pp. Illustrated. 5s.*

This is the fourth edition and with the various impressions the total is eleven issues. All of which adds up to a lot of revision and the high standard that has to be maintained to satisfy the five bodies recommending the book must entail almost a full-time job.

A PICTURE HISTORY OF FLIGHT. *J. W. R. Taylor. Hulton Press, London, 1955. 192 pp. 648 photographs. 25s.*

This book is one of a series of Picture Histories and is a companion volume to *Ballet* by Arnold Haskell. The author has covered the story of man's conquest of the air from the early references of Roger Bacon to the preparatory work on interplanetary flight in six hundred and forty eight photographs.

Additions to the Library

*A.R.C. TECHNICAL REPORTS OF THE A.R.C., VOLS. I-IV, 1945. H.M.S.O. 1955.

Allward, M. F. and Taylor, J. W. R. ROCKETS AND SPACE TRAVEL. Ian Allan. 1956.

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U.S. Weather Bureau. WEATHER REPORTS FROM PILOTS (AVIATION SERIES NO. 14). U.S.G.P.O. 1956.

Wilson, W. Ker. PRACTICAL SOLUTION OF TORSIONAL VIBRATION PROBLEMS: VOL. I. FREQUENCY CALCULATIONS. 3RD EDITION. Chapman & Hall. 1956.

*YEARBOOK OF THE UNIVERSITIES OF THE COMMONWEALTH. Association of Universities of the British Commonwealth. 1956.

ZAEHRINGER, A. J. SOLID PROPELLANT ROCKETS. American Rocket Co. 1955.

**Items marked thus are for reference use only.*

Reports

AERODYNAMICS

Boundary Layer

Nouvelle méthode approchée de calcul des couches limites laminaire et turbulente en écoulement compressible. A. Walz. Pub. sc. et tech. No. 309 (1956).—(1.1.2.2).

Boundary-layer growth and shock attenuation in a shock tube with roughness. P. W. Huber and D. R. McFarland. N.A.C.A. T.N. 3627 (1956).

Measurements of unsteady flow turbulent boundary layer characteristics using a new bullet technique involving study of the bullet bow-wave shape and of shock-wave attenuation have been made in a shock tube with wall roughness.—(1.1.3.4 × 1.2.3.2).

Compressible Flow

See also Boundary Layer

The theoretical wave drag of open-nose axisymmetrical forebodies with varying fineness ratio, area ratio and nose angle. J. H. Willis and D. G. Randall. C.P. 245 (1956).

Existing results for the wave drag of open-nose axisymmetrical forebodies are for bodies whose profiles are straight lines or parabolic arcs. These results are here extended to a family of profiles which includes the straight line and the parabolic arc as special cases. Slender body theory is used throughout.—(1.2.3).

On the one-dimensional refraction of a rarefaction wave at a contact surface. I. J. Billington and I. I. Glass. UTIA Report No. 31 (*April* 1955).

A solution is presented for the one-dimensional refraction of a rarefaction wave at a contact surface in terms of the incident wave strength, the internal energy ratio across the contact surface and constant specific heat ratios.—(1.2.3.2).

Arrangement of fusiform bodies to reduce the wave drag at supersonic speeds. M. D. Friedman and D. Cohen. N.A.C.A. Report 1236 (1955).

Using linearised slender-body theory and reverse-flow theorems, the wave drag of a system of fusiform bodies at zero angle of attack and supersonic speeds is studied to determine the effect of varying the relative location of the component parts. (This report supersedes R.M. A51I20; T.N. 3345).—(1.2.3).

Control Surfaces

See also Wings and Aerofoils

Hinge-moment derivatives for an oscillating control. C. S. Sinnott. R. & M. 2923 (1955).

The semi-empirical "equivalent profile" method of W. P. Jones (1948) is extended to the case of an aerofoil with an oscillating control. The oscillatory hinge-moment derivatives for such an aerofoil-control combination in a low-speed wind tunnel are estimated, an allowance for tunnel wall interference effects being included. A comparison with measured values of the control derivatives is made for two values of the control chord ratio, representing an aileron and a tab. The method of this report gives results in much better agreement with experiment than those obtained by vortex-sheet theory.—(1.3.0).

Theoretical analysis of linked leading-edge and trailing-edge flap-type controls at supersonic speeds. E. C. Yates. N.A.C.A. T.N. 3617 (1956).

The use of linked leading edge and trailing edge flap-type controls for the purpose of reducing hinge moments at supersonic speeds has been analytically investigated. A series of linked controls with supersonic leading and trailing edges on swept and unswept wings has been studied for Mach numbers of 1·414 and 1·960 by use of the linearised theory of supersonic flows.—(1.3.0).

Fluid Dynamics

Analysis of Reynolds number effects in fluid flow through two-dimensional cascades. D. J. K. Stuart. R. & M. 2920 (1955).

An investigation is made into the effects of Reynolds number on the flow through two-dimensional cascades of the diffusing type. Particular emphasis has been placed on the causes of high loss especially at very low Reynolds numbers. Separation of both the laminar and turbulent boundary layers are verified as sources of low efficiency in this particular type of flow and these phenomena have, consequently, been studies in considerable detail. The main work consists of approximate mathematical analysis of representative flows but this theoretical work has been carried out in conjunction with, and is supported by, results drawn from an extensive programme of tests made in the low-speed tunnel at the Cambridge University Engineering Laboratory. Mach number effects were specifically avoided in the experimental work although a wide range of Reynolds number ($3{\cdot}0\times10^4$ to $5{\cdot}0\times10^5$) was covered; the effects of compressibility have not, therefore, been considered in the main analysis.—(1.4.2).

*Sur un curieux cas d'alternance de tourbillons. E. A. Brun. et al. Pub. sc. et tech., No. N.T.*58 (1956).—(1.4).

Formation of a vortex at the edge of a plate. L. Anton. N.A.C.A. T.M. 1398 (*March* 1956).

The flow about the plate of infinite width may be represented as a potential flow with discontinuity surfaces which extend from the plate edges. For prescribed form and vortex distribution of the discontinuity surfaces, the velocity field may be calculated by means of a conformal representation. One condition is that the velocity at the plate edges must be finite. However, it is not sufficient for determination of the form and vortex distribution of the surface.—(1.4.3).

Internal Flow

Theoretical loss relations for low-speed two-dimensional-cascade flow. S. Lieblein and W. H. Roudebush. N.A.C.A. T.N. 3662 (*March* 1956).

A theoretical analysis is conducted of the relations existing between total-pressure defect and wake momentum thickness and form factor for the incompressible flow across a two-dimensional cascade. Both the loss at a plane of measurement and the complete loss after mixing are considered. The relative importance of the various factors entering the loss relations is evaluated. Relations are obtained for the mixing-loss ratio and for the effect of trailing edge thickness.—(1.5.4).

Preliminary investigation of a family of diffusers designed for near sonic inlet velocities. R. Scherrer and W. E. Anderson. N.A.C.A. T.N. 3668 (*February* 1956).

Empirical equations defining a family of diffusers were evolved and several shapes were tested with both separated and unseparated initial boundary layers. The results indicate these diffuser shapes to be satisfactory; however, initial boundary layer thickness was found to be as important as duct shape.—(1.5.1).

Stability and Control

See also Wings and Aerofoils; Helicopter Aerodynamics

Full scale spinning tests on the Percival Provost Mk. 1 *including the inverted spin. T. H. Kerr. C.P.* 240 (1956).

Instrumented spinning tests on this aircraft were completed in both the normal and inverted attitudes. The normal spin showed the characteristics of the smooth and oscillatory type depending upon the control configuration; being oscillatory with pro-spin aileron and smooth with anti-spin aileron. The recovery was satisfactory in each case. Inverted spins of up to six turns were completed and showed satisfactory characteristics both for the spin and the recovery.—(1.8.3).

NOTE.—*The figures in parenthesis at the end of each Summary are for office use only.*

Low-speed wind-tunnel tests on a model of a jet tailless aircraft. J. Trouncer and G. F. Moss. R. & M. 2843 (1956).

The results are given of longitudinal and lateral stability tests made on a model of a jet tail-less aircraft. It includes the effects of split flaps, trimming flaps, dive-recovery flaps and four types of anti-tip-stalling device (slats, nose flaps, double split flaps and letter-box slots). It also includes the effect of the ground in the landing condition.—(1.8.0.2).

Effects of wing position and fuselage size on the low-speed static and rolling stability characteristics of a delta-wing model. A. Goodman and D. F. Thomas. N.A.C.A. Report 1224 (1955).

Results are presented of an investigation made to determine the effects of wing position and fuselage size on the low-speed static and rolling stability characteristics of models having a triangular wing and vertical tail surfaces. Interference increments between the various components are evaluated, and the variation of the vertical tail lift-curve slopes and the efficiency factors with angle of attack as affected by wing position and body size are presented. Tuft-grid pictures of the flow at the vertical tail as affected by wing-fuselage interference are also presented. (This report supersedes T.N. 3063).—(1.8.0).

Determination of lateral-stability derivatives and transfer-function coefficients from frequency-response data for lateral motions. J. J. Donegan. et al. N.A.C.A. Report 1225 (1955).

A method is presented for calculating the lateral stability derivatives and lateral transfer function coefficients from frequency response data for a rigid aeroplane. Two examples are used to illustrate the method. Accurate lateral stability derivatives can be obtained only when accurate frequency response data are available. (This report supersedes T.N. 3083).—(1.8.1).

Theoretical study of the lateral frequency response to gusts of a fighter airplane, both with controls fixed and with several types of autopilots. J. J. Adams and C. W. Mathews. N.A.C.A. T.N. 3603 (1956).

A theoretical approach is used to determine the lateral frequency response of a fighter aeroplane to side and rolling gusts at a Mach number of 0·7 and an altitude of 30,000 feet. The frequency response to gusts is determined for the aeroplane with controls fixed and for the aeroplane in combination with three different basic types of attitude auto-pilots. The power spectral density of the motion of the aeroplane in response to gusts is also presented. A discussion of the response of the aeroplane to command signals is included.—(1.8.0.1).

Analytical study of modifications to the autopilot of a fighter airplane in order to reduce the response to side gusts. C. W. Mathews and J. J. Adams. N.A.C.A. T.N. 3635 (*March* 1956).

Results of some calculations of the roll and yaw responses to side gusts of an aeroplane-auto-pilot system are presented. Course responses to lateral-steering commands are also presented. A comparison is made between the basic auto-pilot and one in which the rudder channel is modified specifically to alleviate the effects of side gusts.—(1.8.1.1).

THERMO-AERODYNAMICS

Correlation of supersonic convective heat-transfer coefficients from measurements of the skin temperature of a parabolic body of revolution (N.A.C.A. RM-10). L. T. Chauvin and C. A. deMoraes. N.A.C.A. T.N. 3623 (1956).

Free-flight tests at supersonic speeds have been made to determine the local convective heat-transfer coefficients, evaluated from measured skin temperatures along the body of a rocket-propelled fin-stabilised parabolic body of revolution (N.A.C.A. RM-10). The Mach number range covered was 1·02 to 2·48 and the Reynolds number range was $3{\cdot}18\times10^6$ to $163{\cdot}85\times10^6$. The experimental values are compared with the results obtained from the V-2 research missile and also with several equations for heat transfer in a turbulent boundary layer.—(1.9.1).

WINGS AND AEROFOILS

See also TESTING AND INSTRUMENTS

Notes on the transonic movement of wing aerodynamic centre. S. B. Gates. R. & M. 2785 (1956).

These notes aim at providing a framework to display what is known of the backward movement of the aerodynamic centre of wing shapes likely to be used for transonic operation, as the flow progresses from incompressible through subsonic to supersonic, the shock-wave régime being ignored. A new geometrical parameter is taken as the main variable because (*a*) it gives a neat classification for the various wing shapes, (*b*) it expresses the results of supersonic theory in a simple form, (*c*) it simplifies the subsonic analysis by making direct use of the similarity law for three-dimensional compressible flow, and so (*d*) it is possible to display on one diagram most of the theoretical and experimental data at present available.—(1.10.2.2).

Aerofoil design in two-dimensional subsonic compressible flow. L. C. Woods. R. & M. 2845 (1955).—(1.10.1.1).

Measurement of lift, pitching moment and hinge moment on a two-dimensional cambered aerofoil to assist the estimation of camber derivatives. H. C. Garner and A. S. Batson. R. & M. 2946 (1955).

Aerodynamic camber derivatives are used in predicting three-dimensional control characteristics, in estimating wind-tunnel interference and in applying model data to full scale. Knowledge of these derivatives has been discussed in R. & M. 2820 (1950), from which it was apparent that experiments were needed to confirm empirical formulae for the derivatives of lift and pitching moment and to check widely differing formulae for the hinge-moment derivative. A two-dimensional R.A.E. 102 aerofoil with a 4 per cent. parabolic centre-line and plain control surfaces of chord ratios 0·2 and 0·4 has been tested at a low speed and Reynolds number $0{\cdot}95\times10^6$. Particular attention is given to the effect of boundary-layer transition. Aerodynamic coefficients are obtained from measured forces and moments and from the pressure distribution at one section.—(1.10.2.1 × 1.3.0).

The calculation of the loading and pressure distribution on cranked wings. G. G. Brebner. R. & M. 2947 (1955).

Using distributions of vortices and sources over the aerofoil surface, approximate formulae are developed for finding the spanwise and chordwise loadings of cranked wings (*i.e.* wings with discontinuous changes of sweep), and the chordwise pressure distribution at the crank, in incompressible flow. The method can be extended to sub-critical compressible flow by considering the "analogous wing." Calculations by the present method are compared with experimental results on two wings and with calculations by another method for wings of *M*, *W* and Λ plan form.—(1.10.1.2).

Tests in the R.A.E. 10-ft.×7-ft. high speed wind tunnel on drop tanks fitted to two swept-back wings. Part I. Comparison of under-wing and wing-tip installations. D. E. Hartley and A. B. Haines. Part II. Comparison of various under-wing drop-tank arrangements. Staff of R.A.E. High Speed Wind Tunnel. R. & M. 2951 (1956).

The report describes tests made in the Royal Aircraft Establishment 10 ft.×7 ft. high speed wind tunnel on drop tanks, fitted to two wings, having the same plan form with a sweepback of 40 deg. and an aspect ratio of 3·5, but differing in thickness/chord ratio, being 10 per cent. and 8·5 per cent. thick respectively.—(1.10.2.2).

*Flight testing of a Vampire aircraft at maximum lift. P. B. Atkins. A.R.L. Note F.*23 (*August* 1955).

Flight tests have been made on an Australian Vampire to assess the effect on boundary layer separation of the fitting of turbulence strips. These tests extend into flight a wind tunnel investigation of the mode of stall of this aerofoil, and consist of (*a*) an investigation of the general flow over the wing tip by means of tufts, (*b*) a pilot's assessment of the change in aircraft behaviour at stall due to the fitting of the turbulence strips, (*c*) detailed measurements of wing surface pressures near the leading edge, (*d*) comparative measurements of aircraft drag and (*e*) a boundary layer traverse just ahead of the aileron with and without strips fitted.—(1.10.2.2).

*The effect of a small laminar separation bubble on the potential flow past an aerofoil. D. G. Hurley. A.R.L. Note A.*148 (*October* 1955).

A simple approximate method is given for calculating the effect that a small bump of arbitrary shape on the surface of an aerofoil has on the surface pressures near it. A

calculation method is also given for the converse problem, *i.e.* the problem of finding the shape of the bump if the changes in pressure that it produces are known.—(1.10.1.1).

Theoretical prediction of pressure distributions on nonlifting airfoils at high subsonic speeds. J. R. Spreiter and A. Alksne. N.A.C.A. Report 1217 (1955).

Theoretical pressure distributions on non-lifting circular-arc aerofoils in flows with high subsonic free-stream velocity are found by determining approximate solutions of an integral equation for transonic flow proposed by Oswatitsch. Solutions are obtained using an iteration process which differs from the classical methods in that the quadratic nature of the integral equation is recognised. (This report supersedes T.N. 3096).—(1.10.1.1).

Effect of ground interference on the aerodynamic and flow characteristics of a 42° *sweptback wing at Reynolds numbers up to* $6{\cdot}8\times10^6$. *G. C. Furlong and T. V. Bollech. N.A.C.A. Report* 1218 (1955).

The effects of ground interference on the aerodynamic and flow characteristics of a 42° swept-back wing have been determined at distances above the ground 0·68 and 0·92 of the mean aerodynamic chord (measured from 0·25 mean aerodynamic chord). The wing was tested without flaps and with inboard trailing-edge split and outboard leading-edge flaps deflected. The wing had an aspect ratio of 4, a taper ratio of 0·625, and N.A.C.A. 64_1-112 aerofoil sections perpendicular to the 0·273 chord line.—(1.10.2.2).

Measurement and analysis of wing and tail buffeting loads on a fighter airplane. W. B. Huston and T. H. Skopinski. N.A.C.A. Report 1219 (1955).

Wing and tail buffeting loads measured in 194 runs with a fighter-type aeroplane are tabulated with the associated flight conditions. Measurements were made at altitudes of 30,000 to 10,000 feet and at Mach numbers up to 0·8. (This report supersedes T.N. 3080).—(1.10.2.2).

A method for the design of sweptback wings warped to produce specified flight characteristics at supersonic speeds. W. A. Tucker. N.A.C.A. Report 1226 (1955).

A method is presented for designing swept-back wings to be self-trimming at a given set of flight conditions. This characteristic is achieved by warping the wing in a particular manner, that is, by giving the wing a certain combination of angle of attack, twist, and camber. The method applies directly to a wide class of swept-back wings. The application to any specific wing is simplified to a routine computational procedure, and a discussion is given of some points to be considered in the application to a practical case. (This report supersedes R.M. L51F08).—(1.10.1.2).

A flat wing with sharp edges in a supersonic stream. A. E. Donov. N.A.C.A. T.M. 1394 (*March* 1956).

A basic treatment is given for the approximate solution of the problem of two-dimensional supersonic flow past a thin wing at small angles of attack. The pressure distribution at the surface, the lifting force, and the wave drag are determined.—(1.10.1.2).

A theoretical study of the aerodynamics of slender cruciform-wing arrangements and their wakes. J. R. Spreiter and A. H. Sacks. N.A.C.A. T.N. 3528 (1956).

A theoretical study is made of the pressures, loadings, forces, and vortex wake associated with certain cruciform wing arrangements. For 45° bank, the wake of a cruciform wing is treated numerically with 40 vortices and analytically with 4 vortices. Comparisons are made with water-tank measurements, and the calculation of loads on cruciform tails by reverse flow is considered.—(1.10.1.2).

Effect of thickness, camber, and thickness distribution on airfoil characteristics at Mach numbers up to 1·0. *B. N. Daley and R. S. Dick. N.A.C.A. T.N.* 3607 (1956).

A modified open-throat type of wind tunnel developed for the purpose of obtaining two-dimensional aerofoil data at Mach numbers near 1·0 is presented and discussed. Tests of a group of related N.A.C.A. aerofoil sections varying in maximum thickness, design lift coefficient, and thickness distribution have ben conducted in this wind tunnel at Mach numbers of 0·3 to about 1·0 and at corresponding Reynolds numbers from $0{\cdot}7\times10^6$ to $1{\cdot}6\times10^6$.—(1.10.1.2 × 1.12.1.2).

Linearized lifting-surface and lifting-line evaluations of sidewash behind rolling triangular wings at supersonic speeds. P. J. Bobbitt. N.A.C.A. T.N. 3609 (*March* 1956).

The sidewash in the vertical plane of symmetry behind steady-rolling triangular wings travelling at supersonic speeds has been derived by use of both linearised lifting-surface and lifting-line methods. The lifting-surface results are limited by the condition that the wing leading edges must be subsonic. Variations of the sidewash with vertical and longitudinal distances are presented in graphical form for a number of combinations of Mach number and leading-edge sweep.—(1.10.1.2 × 1.8.2.2).

Experimental investigation of the flow around lifting symmetrical double-wedge airfoils at Mach numbers of 1·30 *and* 1·41. *B. B. Gooderum and G. P. Wood. N.A.C.A. T.N.* 3626 (*March* 1956).

Measurements were made of the flow around a 10 per cent. thick, doubly symmetrical, two-dimensional wedge at a Mach number of 1·30 and a 14·2 per cent. thick wedge at Mach numbers of 1·30 and 1·41 for various angles of attack up to 5°. Pressure and Mach number distributions, lift and drag coefficients, centre of lift, and pitching moment are presented for the angles of attack used.—(1.10.2.1).

Investigation of the effects of ground proximity and propeller position on the effectiveness of a wing with large-chord slotted flaps in redirecting propeller slipstreams downward for vertical take-off. R. E. Kuhn. N.A.C.A. T.N. 3629 (1956).

Results are presented of an investigation of the effects of proximity to the ground on the effectiveness of large-chord slotted flaps in redirecting propeller slipstreams downward for vertical take-off. The model consisted of a semi-span wing equipped with a 60 per cent. chord and 30 per cent. chord slotted flap operating in the slipstream of two large-diameter propellers. The tests were conducted at zero forward speed in a large room.—(1.10.2.2).

Hovering-flight tests of a model of a transport vertical-take-off airplane with tilting wing and propellers. P. M. Lovell and L. P. Parlett. N.A.C.A. T.N. 3630 (1956).

An investigation of the take-off, landing, and hovering-flight characteristics of a four-engined transport vertical-take-off aeroplane with tilting wing and propellers has been conducted with a remotely controlled free-flight model. The model had four propellers distributed along the wing with the thrust axes in the wing-chord plane. To produce direct lift for hovering flight with the fuselage horizontal, the wing and propellers were rotated 90° relative to the fuselage.—(1.10.2.2).

Helicopter Aerodynamics

Charts for estimating tail-rotor contribution to helicopter directional stability and control in low-speed flight. K. B. Amer and A. Gessow. N.A.C.A. Report 1216 (1955).

Theoretically derived charts and equations are presented by which tail-rotor design studies of directional trim and control response at low forward speed can be conveniently made. Comparisons between theoretical and experimental results are presented.—(1.11.2).

Charts for estimating rotor-blade flapping motion of high-performance helicopters. R. J. Tapscott and A. Gessow. N.A.C.A. T.N. 3616 (1956).

Theoretically derived charts of flapping coefficients are presented for helicopter rotors having blades with a linear twist of 0°, -8°, and -16°. The charts, showing rotor flapping coefficients for combinations of inflow ratio and blade pitch angle, are presented for tip speed ratios ranging from 0·05 to 0·50.—(1.11.3).

Wind-tunnel investigation of effects of fuselage cross-sectional shape, fuselage bend, and vertical-tail size on directional characteristics of nonoverlap-type helicopter fuselage models without rotors. J. L. Williams. N.A.C.A. T.N. 3645 (*March* 1956).

The results are presented of a low-speed wind tunnel investigation to determine the effect of fuselage cross section, fuselage bend, and vertical tail size on the directional stability characteristics of non-overlap-type helicopter fuselage models.—(1.11.2 × 1.8.1.2).

TESTING AND INSTRUMENTS

See also WINGS AND AEROFOILS

Development of an air mass-flow rate meter. W. J. G. Cox. C.P. 230 (1956).

The development of an air mass flow rate meter to cover a very wide range is described, which, essentially an analogue computor, gives a two-sweep pointer direct presentation of air mass flow rate, independent of pressure, temperature and velocity changes within the range of the instrument. The pointers are driven by a servo-system which is error-actuated from the computing bridge network, secondary feed-back being used to maintain stability with a saturated angular output rate of approximately 33° per second. Specifications and performance figures are given for the individual transducer elements and the complete instrument, error estimations are made, and the servo stability is discussed.—(1.12×27).

An automatic self-balancing capsule manometer. G. F. Midwood and R. W. Hayward. C.P. 231 (1956).

A brief description is given of an automatic self-balancing capsule manometer which has been developed primarily for the measurement of pressures encountered in the operation of supersonic wind tunnels. A remote indicating system is used which is suitable for use with digital recording and card punching apparatus.—(1.12.5).

A technique for the measurement of pressure distribution on oscillating aerofoils, with results for a rectangular wing of aspect ratio 3·3. *W. G. Molyneux and F. Ruddleston. C.P.* 233 (1956).

Details are given of a strain gauge pressure transducer that has been developed for measurements of pressure distribution on oscillating aerofoils in low-speed wind tunnels. The transducer characteristics are shown to be well suited to oscillatory measurements, and in particular the transducer output can be measured directly on a sensitive galvanometer without the need for preamplification. As an illustration of the use of the transducer, pressure measurements have been made in the R.A.E. 5 ft. diameter open jet wind tunnel on a rectangular wing of aspect ratio 3·3 oscillating in modes of pitch and roll.—(1.12.6.1×1.10.2.2).

Low speed wind tunnel calibration of a Mk.9A pitot-static head. J. E. Nethaway. C.P. 244 (1956).

The calibration of a Mark 9A Pitot-static head in the No. 2 11½ feet wind tunnel is described. The results show the variation of Pitot and static pressure error coefficients with incidence, at constant tunnel speed.—(1.12.2×13).

Theoretical requirements of tunnel experiments for determining stability derivatives in oscillatory longitudinal disturbances. S. Neumark and A. W. Thorpe. R. & M. 2903 (1955).

The need of using stability derivatives of unsteady (oscillatory) motion is explained, and requirements of tunnel experiments for determining them are established. These are based on simple theoretical considerations, valid for both incompressible and compressible (sub- and supersonic) flow. Two alternative schemes of experimental tests are critically examined. Non-dimensional derivatives are defined and applied in modified stability equations, referred to fixed or moving systems of co-ordinate axes.—(1.12.1).

The hot-wire anemometer and its use in non-steady flow. I. J. Billington. UTIA Technical Note No. 5 (*September* 1955).

The thermal equilibrium and heat loss relationships for small diameter heated cylinders placed normal to an air-stream are discussed. Some experimental results are presented for the heat loss of a hot-wire in stationary gases. Procedures for the calibration of hot-wires are given in some detail and typical calibration curves are shown. The design of an open-jet hot-wire calibration tunnel with a variable velocity and a low turbulence level is described.—(1.12.6.3).

Investigation of the time response and outgassing effects of pressure probes in free molecule flow. E. L. Harris. UTIA Technical Note No. 6 (*October* 1955).

A study was made of the time response and outgassing effects of pressure probes in free molecule flow. A theoretical analysis was made to obtain expressions for the time constant of a pressure probe, and for the pressure difference due to outgassing between the gas in the gauge and the gas external to the gauge under steady conditions. The expression derived for the time constant was in terms of the physical dimensions of the gauge; that for the pressure difference was in terms of the physical dimensions and a factor q, which was the rate at which adsorbed gas was evolved from the surface of the gauge per unit area.—(1.12).

Étude expérimentale des écoulements gazeux. M. Merle. Pub. sc. et tech., No. 308 (1956).—(1.12.0).

Theoretical study of the tunnel-boundary lift interference due to slotted walls in the presence of the trailing-vortex system of a lifting model. C. W. Matthews. N.A.C.A. Report 1221 (1955).

The equations which represent the interference on the lift of uniformly loaded wings of finite span in circular tunnels with mixed open and closed boundaries are derived, with special attention to those tunnels containing symmetrical arrangements of the open and closed portions. The equations are applicable to tunnels with other cross-sectional shapes, provided a transformation function can be found which will transform the tunnel cross section into a circle. (This report supersedes R.M. L53A26).—(1.12.1).

A free-flight wind tunnel for aerodynamic testing at hypersonic speeds. A. Seiff. N.A.C.A. Report 1222 (1955).

Research models are fired from a gun through the wind tunnel test section in a direction opposite to the air stream which has a moderate supersonic Mach number. A wide range of Mach numbers, from low supersonic speeds up to $M = 10$, can be obtained. The equipment and test techniques are described. The methods used to measure drag, lift-curve slope, centre of pressure, and damping in roll are given. The imperfections in the air stream and their effect on model tests are discussed. (This report supersedes R.M. A52A24).—(1.12.1.3).

Investigation of the use of the thermal decomposition of nitrous oxide to produce hypersonic flow of a gas closely resembling air. A. P. Sabol and J. S. Evans. N.A.C.A. T.N. 3624 (1956).

A method is presented for producing hypersonic flow in a wind tunnel by using the hot gaseous products obtained from the thermal decomposition of nitrous oxide. Comparable experimental results were obtained when pressures in a divergent conical hypersonic nozzle operated by this method at a Mach number of about 7 were compared with those of heated air. The design of the reactor used to operate the nozzle was based on results of constant-volume and constant-pressure decomposition tests which are described.—(1.12.1.3).

AEROELASTICITY

Wind-tunnel flutter tests on a model delta wing under fixed and free root conditions. D. R. Gaukroger. et al. R. & M. 2826 (1955).

Wind tunnel tests on a model wing of delta plan form are described. Tests have been made with the wing root fixed and also with the root free in pitch and vertical translation. Critical flutter speed and frequency are given for a wide range of variation of wing and fuselage inertia parameters.—(2).

Rolling power measurements and comparative calculations for a flexible model wing constructed from Xylonite. N. C. Lambourne, et al. R. & M. 2895 (1955).

Partly to gain experience of aeroelastic models constructed from Xylonite, and partly to provide information regarding loss of rolling power due to wing distortion, a tip-to-tip model of the wings of a Spitfire aircraft was constructed. It was mounted on a longitudinal axis in a wind tunnel so that it could roll continuously. Rates of rolling were measured for a range of air speeds and the results are compared with those of calculations.—(2).

Wind-tunnel tests on the symmetric and antisymmetric flutter of swept-back wings. D. R. Gaukroger. R. & M. 2911 (1955).

Wind tunnel tests to determine the symmetric and anti-symmetric flutter characteristics of a swept-back wing are

described. The investigation covers the separate experimental treatment of the symmetric and antisymmetric body freedoms over a range of wing sweepback angles. Consideration is also given to the effect on critical flutter speed and frequency of variations in overall centre of gravity position, fuselage pitching moment of inertia, fuselage rolling moment of inertia, fuselage mass and tailplane volume coefficient.—(2).

Preliminary study of some factors which affect the stall-flutter characteristics of thin wings. A. G. Rainey. N.A.C.A. T.N. 3622 (1956).

The results of an exploratory, analytical, and experimental study of some of the factors which might be of importance in the stall flutter of thin wings are presented. The factors considered were Mach number, Reynolds number, density, aspect ratio, sweep-back, structural damping, location of torsion nodal line, and concentrated weights.—(2.0).

ELECTRONICS

Die Verwendbarkeit eines elektronischen Zählgerätes zur Messung von Mengen, Zeiten und Frequenzen sowie zur Impulsgabe. A. Becker. D.F.L. Bericht Nr. L46-55/9. (*October* 1955).

In dem vorliegenden Bericht wird der Aufbau einer Messeinrichtung zur Mengenmessung, Zeitmessung, Impulsgabe und Frequenzmessung beschrieben. Die angeführten Messbeispiele zeigen die Anwendung der universellen Anlage für einige bestimmte Messaufgaben.—(11).

FLIGHT TESTING

See AERODYNAMICS—TESTING AND INSTRUMENTS

FUELS AND LUBRICANTS

Étude de la lubrification sous pression rapidement variable. Cas des lubrifiants plastiques; existence de leur anisotropie. F. Charron. Pub. sc. et tech., No. 310 (1956).—(14.0).

HYDRODYNAMICS

Effect of carriage mass upon the loads and motions of a prismatic body during hydrodynamic impact. M. F. Markey. N.A.C.A. T.N. 3619 (1956).

Theoretical equations for the effect of a towing-carriage mass upon the loads and motions of a prismatic body during a hydrodynamic impact are presented. These equations apply to any prismatic-body impact depending only upon the variables substituted.—(17.2).

MATERIALS

See also STRUCTURES—THEORY AND ANALYSIS

Colles a base de polyuréthanes. J. Grosmangin. O.N.E.R.A. Note Technique No. 29.—(21.3.3).

MATHEMATICS

*The solution of high order polynomial equations by matrix iteration. J. Guest. A.R.L. Report SM.*236 (*December* 1955).

Aitkin's method for finding the dominant root of a polynomial equation has been extended to find all its roots. As in Aitkin's case, the roots are found by iteration.—(22.1).

Les applications scientifiques de la notion de régularité. P. Vernotte. Pub. sc. et tech., No. 307 (1956).—(22.1).

METEOROLOGY

Report of the first year's flying on the development of flight testing techniques for finding and measuring natural icing conditions. G. C. Abel. C.P. 221 (1956).

The results of the first year's flying in search of natural icing conditions using a Viking aircraft are given. Special meteorological forecasts of icing conditions have proved to be reasonably reliable. Good photographs of droplets that have been sampled from a cloud can now be obtained at will and they give direct measurement of droplet sizes although, when frozen particles and supercooled droplets are present together, the interpretation of the photograph is still subject to doubt.—(24).

Report of the third year's flying on the development of flight testing techniques for finding and measuring natural icing conditions. G. C. Abel. C.P. 223 (1956).

This report covers the third year's search for natural icing conditions together with some results for the whole three year period. Various forms of unserviceability limited this year's flying to approximately four months. For the three year period only half the icing forecasts for layer type cloud over U.K. yielded conditions sufficiently continuous to allow an anti-icing system to be tested. A number of these were too limited to allow more than one test run.—(24).

Reference pressure probes for an orifice-type icing detector. D. Fraser and D. C. Baxter. N.A.E. Note 12 (1956).

The operation of an orifice-type icing detector depends on having a suitable reference pressure source. A flush-type static vent may not provide this, especially when the detector is used in an engine intake duct. However, tests show that a production form of icing detector probe can be modified to provide a suitable reference pressure if it is de-iced or anti-iced. A reference probe which is particularly insensitive to yaw is also described.—(24).

POWER PLANTS

See also AERODYNAMICS—TESTING AND INSTRUMENTS

The aerodynamics of the cooling of aircraft reciprocating engines. A. S. Hartshorn and L. F. Nicholson. A.R.C. Monograph. R. & M. 2498 (1956) (*Monograph*).

This monograph gives an account of the work done at the Royal Aircraft Establishment during the period 1939-1946 on the aerodynamics of cooling. Reference is also made to work performed by the Bristol Aeroplane Co. and to information of overseas origin. It is divided into 12 chapters each dealing with a specific part of the general problem. These chapters do not give the whole of the work done but show in a general way the historical progress and indicate the present opinion.—(27.2).

Schwingungsverhalten eines Schaufelkranzes im Vakuum. H. Söhngen. D.V.L. Bericht Nr. 1 (*August* 1955).

Untersucht werden die Eigenschwingungs-Formen und Frequenzen eines Schaufelkranzes, dessen Schaufeln über den Fuss hinweg gekoppelt sind. Das Schwingungsverhalten der einzelnen Schaufel wird dabei als bekannt vorausgesetzt und nur das Verhalten des Schaufelverbandes betrachtet, und zwar für die Fälle, dass

(*a*) sämtliche Schaufeln die gleiche Frequenz haben,

(*b*) Schaufeln zweier Frequenzgruppen abwechseln.—(27.1).

The design of a miniature solid-propellant rocket. R. H. Heitkotter. N.A.C.A. T.N. 3620 (1956).

A miniature rocket motor was designed and developed to exert 3 ounces of thrust for a duration of 2 seconds. The rocket has reproducible performance and is simply constructed. The research that led to the development of the miniature rocket showed that compensatory measures must be used in designing extremely small rockets because of excessive heat-transfer rates.—(27.3).

STRUCTURES

LOADS

Loading conditions following an automatic pilot failure (rudder channel). D. R. Puttock. C.P. 242 (1956).

A method is presented for the determination of the critical loading conditions of aircraft that ensue from an automatic pilot failure in the rudder channel. An example is given to illustrate the type of response produced by a rudder channel failure and the calculation procedure.—(33.1.1).

Loading conditions following an automatic pilot failure (elevator channel). D. R. Puttock. C.P. 243 (1956).

A proposal is made for a standard procedure for calculating the critical loading conditions ensuing from an automatic pilot failure in the elevator channel. The influence of the sequence of elevator movements on the loading conditions is discussed with reference to an example.—(33.1.1).

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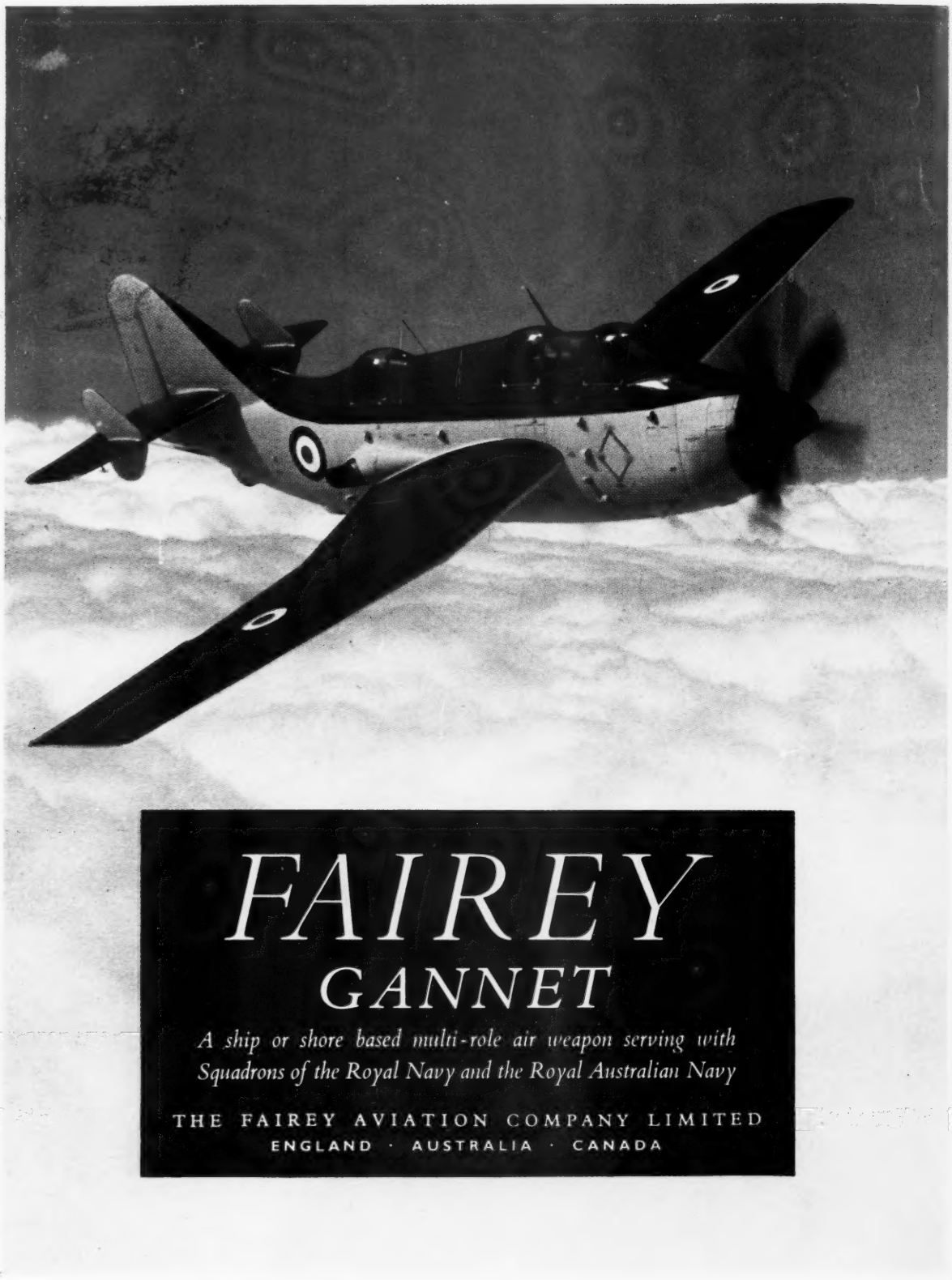
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